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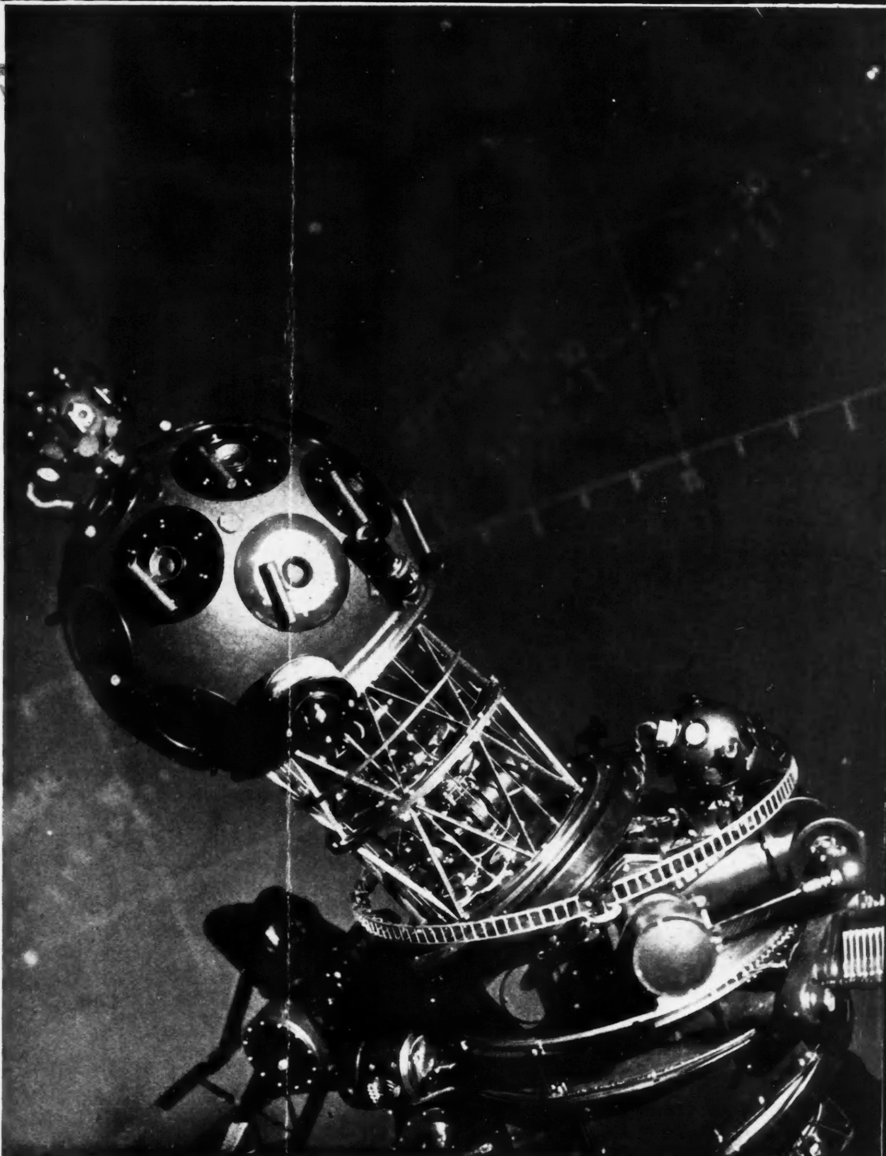

Star projector of Hayden Planetarium

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THE MAGAZINE OF SCIENTIFIC PROGRESS

April, 1946 Vol. VII. No. 4

Editor: WILLIAM E. DICK, B.Sc.
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The Progress of Science

Crossroads Ahead

It is about three months since the United Nations passed a resolution setting up a Commission 'to deal with the problems raised by the discovery of atomic energy and other related matters'. The Commission was instructed to 'proceed with the utmost dispatch'—yet the public is still awaiting news of its activities. In fact almost the only important news that we have come across is of the appointment of the representatives of various powers—Sir Alexander Cadogan for Britain, with Sir James Chadwick as his second string—and the fact that observers from the Commission are to be present at the forthcoming American atomic bomb experiments. This hardly seems to indicate a favourable interpretation of the words 'utmost dispatch'.

The resolution setting up the Commission at least provided scope for a reasonably rapid solution of the problem. The Commission, readers will remember, was to 'inquire into all phases of the problem', and apart from the general tone of the resolution it was particularly instructed to make proposals for the solution of four specially important problems, namely: the abolition of secrecy in scientific matters; the confining of the use of atomic energy to peaceful purposes; the elimination of atomic weapons from national armaments; and the devising of control and inspection methods.

It was a good resolution. But good resolutions in themselves contribute very little to the solution of problems of international rivalry. Everything depends on how they are interpreted.

It was clear from the beginning that, while there are forces in the world who will press for a liberal interpretation which would turn the words into practice, yet there are also forces who will use every possible means to obstruct the execution of the resolution. The lapse of four months without important news of progress would seem to indicate that the latter forces have carried the greater influence, at least temporarily.

Most outspoken among the obstructing forces were the isolationists in the U.S.A. They saw the resolution, not as an opportunity to solve the problem of security in general, but as a threat to the security of the United States in particular. The political agitation which followed was concerned almost entirely with attempting to secure from U.S. statesmen assurances that no atomic bomb or atomic energy secrets held by the U.S.A. would be divulged to the Commission. The resolution had been drafted jointly by the foreign ministers of Great Britain, the U.S.A. and the U.S.S.R. Yet within a fortnight, Mr. Byrnes, apparently in response to isolationist agitation, was expressing such sentiments as "I do not see how the language used can possibly be construed to give the Commission authority to obtain information not voluntarily given to it", and saying that America's voting for the resolution "could not give the Commission authority to decide what information the United States or any other Government should place at its disposal".

It is, of course, obvious that the Commission is given no powers to compel states to divulge information; and it is equally true that at the present stage in the life of UNO no resolution could compel a major power to do anything against its will. Nevertheless the success of the Commission's work will depend very largely on how much the powers concerned will voluntarily yield up to it the information, technical and otherwise, that it requires. An essential condition for controlling anything, as any scientist knows, is a full knowledge and understanding of it. It would seem that the first step towards world security in regard to atomic fission is to achieve joint control of it by the United Nations, which implies joint knowledge of it by the United Nations or by some representative body in which the member states place full confidence. On what other basis could the Commission work out, for example, measures "for effective safeguards by way of inspection and other means to protect complying states against the hazards of violations and evasions."?

In addition, the success of the Commission will depend very largely on the extent to which America succeeds in convincing other states that her motives are entirely unselfish. However good American intentions may in fact be, they must necessarily appear as highly suspicious to other countries, so long as secrecy is maintained. There will inevitably be the suspicion that the secret is being kept as a bargaining force to further American interests in the

many diplomatic discussions that will go on in the next two or three years. Unfortunately tacit acceptance of the American policy involves this country at least partly in those suspicions.

Any suspicions that the rest of the world, justifiably or not, may harbour cannot but be intensified by the fact that, even while supporting resolutions designed to bring security through international action, America is about to embark upon what is probably the largest-scale military experiment ever carried out. There is a symbolic tang about the code name of the experiment—Operation Crossroads. In July and at two subsequent dates, Nagasaki-type atomic bombs (already considered to be obsolete) are to be dropped on groups of ships in a lagoon in the Marshall Islands. Altogether 97 ships are to be destroyed in these experiments; some 20,000 men are to be employed on them for several months; and the cost is estimated at £125,000,000.

The earlier announcements of these tests sounded ominous in the extreme. Here was "*Si vis pacem, para bellum*" on the scale of atomic war. Vice-Admiral Blandy, who is directing the tests, stated the object simply as to "gain information of value to the national defence, while a secondary purpose would be to give Army airmen training in attacking warships with atomic bombs", and emphasised that "the tests were scientific experiments by the United States alone, and not a combined international operation." Since then, however, two events have slightly relieved the gloom. The first test has been postponed from its original date in May to one in July, in order to avoid a conflict of dates with the UNO meeting in New York; and it has been announced that observers from the United Nations Commission are to be permitted to be present. This at least indicates some recognition that tests on the atomic bomb, far from being the concern of the United States alone, are the concern of all nations of the world. And the presence of observers from the Commission will give the work of that body some degree of the concreteness which it has hitherto lacked.

But even this concession must not be valued too highly. In a large-scale experiment of this kind, it is access to the detailed analysis of the results that counts. Mere observers will pick up little more than material for a couple of columns in a newspaper, unless they are permitted to follow through the whole process of analysis that will follow the experiment.

Nevertheless the projected presence of these observers at the tests must be grasped as the one straw indicating some hope that the veil of secrecy which is at present causing so much international distrust is to be lifted. Lifted it must be, and soon, if there is to be any relief from the suspicions that so much afflict the world and any hope of the permanent solution of other international problems.

It is indeed discouraging that there has been no widespread movement among scientists in this country against the present tendency towards greater and stricter national secrecy in scientific affairs. Across the Atlantic the Federation of American Scientists (formerly the Federation of Atomic Scientists) has been conducting a virile public campaign by every means at its disposal for the quick lifting of secrecy restrictions. Its members have been preaching widely the fallacy of the belief that atomic (or any other scientific) secrets can be kept for more than a very few years, and the awful consequences that will follow if each nation is compelled to investigate these things in competitive isolation. They claim to have reached between 10 and 40 million people with their message. In this country only a few lone voices have cried out in protest. In this connexion the Association of Atomic Scientists, which is being set up in Britain, has a colossal task ahead since it has so much leeway to make up.

The lethargy of British scientists in the immediate past is the more difficult to understand and condone in view of the fact that it is becoming only too evident that the secrecy is by no means confined to matters of atomic weapons or even of techniques which are primarily warlike, that it threatens to engulf very wide fields of scientific research. It has become clear that a scientist can be faced with serious charges for revealing nuclear data which one might think to belong to the field of fundamental science, and not of atomic bomb technology. Again, the sphere in which Anglo-American secrecy tends to operate to the exclusion of other countries covers important aspects of research on, for example, radar* and penicillin. (Surely there can be no excuse for the inhumanity of preventing the most rapid dissemination of details about the latter.) Even the scientists who do not care to meddle in national or international politics should be awakened to action by this threat to the freedom of scientific communication over so wide a field that, should it continue for another year or two, there will be little left free from the threat of the Official Secrets Act but nature study.

While on the subject of secrecy, it would be well to reiterate one fact that is too often politely glossed over in discussions of atomic bomb politics: namely, that the effective secrets of atomic bomb manufacture and the more advanced development work on atomic energy are not equally shared by the wartime partnership of the U.S.A., Great Britain and Canada. On the research side Britain and Canada played a big part, but they have been excluded from much of the knowledge of actual industrial processes. On February 6 Mr. Attlee confirmed a suspicion that has long been current, when he said that no British scientist has yet visited the Hanford engineering works, which is one of the two main American production plants.

In this uneasy world it is the atomic bomb that gets the headlines. Atomic energy for power purposes gets pushed into the background. Definite information about the scale of work on peaceful atomic energy in the U.S.A. is almost entirely lacking, though there has been a statement that America intends to publish details about atomic developments with peacetime uses. And at home the most persistent questioning in the House of Commons on the subject of the proposed atomic research station at Didcot has yielded very little positive information beyond the facts that its director is Professor Cockroft and that the expenditure is expected to be £2,800,000 for the year 1946-47. The sum of £2,800,000 on peacetime uses (or is it *all* uses?) of atomic energy in Britain seems very small when contrasted with the £125,000,000 that Operation Crossroads will cost.

If one adds the fact that up till the end of January the

* A 25-volume work on radar is rumoured to be under preparation in the U.S.A.; if that does prove to be true then a great deal of information is coming off the secret list. The London Radar Convention of last month has already released many details.

Government was not prepared to make any statement as to when building operations at Didcot are likely to begin and has not volunteered any statement since, one has almost the sum total of the information that has been made available to the public concerning British policy on atomic research. (It is to be regretted, by the way, that the setting up of the United Nations Atomic Commission is now being used as an official excuse for withholding information on these matters.)

One thing seems clear: there is a complete lack of understanding in official circles and among the general public that if the practical realisation of the peaceful uses of atomic energy is going to take (say) ten years, then it will take ten years from the time at which research is started. Perhaps the old hope is being revived, in spite of many experiences that have proved its falsity, that we can import the research results of others, when these are completed, and merely apply them as a going concern. If that hope is in any way the basis of official policy, it must be quickly dispelled.

Applications of Echo Sounding

ECHO sounding is best known today for its role, in the form of ASDIC, in defeating the U-boats. Its uses in peace are less celebrated. Yet it has served fishing fleets by detecting shoals of fish, and it guided many a ship in fog while radar was still undeveloped.

The technique of echo sounding was described in the first of our two articles on 'Science in the Naval War' (December 1945, pages 363-5), and more particulars were given in the article by Dr. E. G. Richardson published in August 1943. Essentially it is a method of determining and recording the distance between a ship and the nearest underwater reflecting surface in a given direction, by measuring the time lapse between the emission of a pulse of super-sonic waves and the return of the echo.

The two pictures which are taken from the new book entitled *Scientific Instruments** are excellent examples of its powers. Fig. 1 shows how distinctly a shoal of fish is portrayed on the ASDIC recorder. ASDIC detection was being used by trawling fleets before the war and there is no doubt that its wider application can considerably increase the efficiency of our fishing industry. Note how clearly the profile of the sea bottom is shown in the same picture. The old method of sounding by line and plummet (with its nineteenth-century refinements) has been one of the principal aids to navigation for centuries, and the captain usually heaved a sigh of relief when he could leave his calculations of latitude and longitude for the more satisfactory determination of position by comparison of his soundings with the chart. Echo sounding, with its continuous and precise record of the contour of the sea bottom, enormously increased the efficiency of sounding as a means of navigation on the approach to land. It will be some years at least before radar becomes so universal an item of equipment as to displace it from that job.

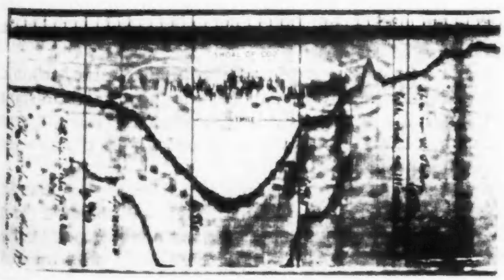


FIG. 1.—An example of what can be done by echo sounding; a shoal of cod shown over a valley at the bottom of the sea.

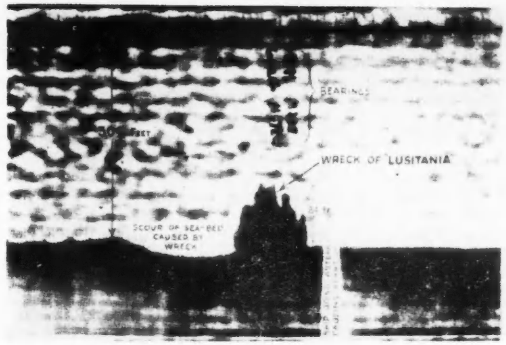


FIG. 2.—The wreck of the *Lusitania*, revealed by echo sounding, in 309 feet of water.

The method has also found very wide application in surveying, in which it is far more rapid and accurate than any mechanical method, and for checking the dredged channels of harbours. It is also used for the detection of wrecks—an application that may, unfortunately, be much in demand in the next two or year—and Fig. 2 shows how accurately it delineates the sunken vessel. (The outline of the ship is, of course, distorted, since the recorder shortens the horizontal scale in relation to the vertical.)

The industrial development of seaweed products will necessitate more scientific mapping of seaweed beds, and it is interesting to note that here again echo sounding is helpful.

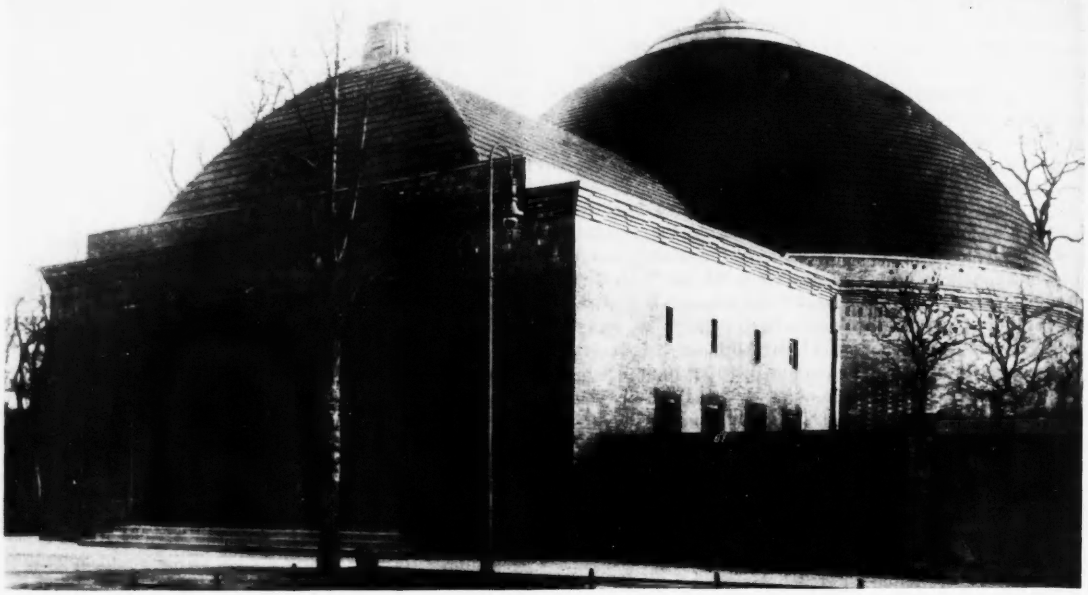
During the war the whole technique of echo sounding has been radically improved by Admiralty research, the results of which still remain largely secret. It is to be hoped that these researches will soon bear new fruit in more accurate and widespread use along lines such as those sketched above.

Britain needs a 'Star House'

SIR A. P. HERBERT recently started a correspondence in the *Sunday Times* advocating the establishment in Britain of at least one planetarium. His letter was supported by another from the Astronomer Royal who mentioned that the planetarium at Moscow had an attendance of almost a million people each year. Britain must be almost the only country in Europe without a single planetarium: even such small countries as Sweden can boast one, while in the United States there are a very considerable number.

A planetarium, may we explain, is a device for producing

* Edited by H. J. COOPER (London, Hutchinson, 1946; 25s.) This 294-page volume is intended for the layman and gives some interesting details about a wide range of scientific instruments.



Exterior view of the Berlin Planetarium.

on the inside of a smooth doomed roof a representation, by means of projected spots of light, of the stars, planets, moon and sun, showing all their motions correctly, either at the natural rate, or greatly speeded up, and with an adjustment so that the latitude from which this artificial sky is seen may be altered at will.

This device is the more complicated offspring of an old toy called the orrery, which consisted of a model, driven by clockwork, showing the motions of the earth and moon round the sun. This took its name from Charles Boyle, Earl of Orrery, kinsman of the Hon. Robert Boyle ('father of chemistry and uncle of the Earl of Cork'). Charles Boyle, who lived in the late seventeenth and early eighteenth centuries, had that quality of intellectual liveliness and versatility, as often associated with trivialities as with matters of moment, which was somehow characteristic of the new aristocracy of that time. He was a soldier, a diplomat and an author, but he is remembered now only because he used to amuse his leisure hours with mechanical toys, of which the orrery was one.

A development of the orrery was a very large scale model constructed in 1913 for the museum at Munich in which the observer was carried round on the model earth, and could look through a miniature telescope at lamps fixed so as to represent the stars. The modern form of the planetarium, developed by Zeiss of Jena, removes the various difficulties of this form and makes it a medium of public entertainment and instruction.

The projection apparatus for the stars is relatively simple. Two hemispheres at opposite ends of a lattice arm constitute the projector which resembles a huge dumb-bell. One 'knob' of the dumb-bell projects the northern stars by means of a series of lenses which produce spots of light in the correct positions: the other knob similarly projects the southern stars. By tilting the dumb-bell, more of the southern stars can be brought into view at the same time as the northern ones disappear, corresponding to a movement of the observer southwards on the surface of the earth. For any fixed position of the dumb-bell a rotation about the line of the bar gives the movements of the stars due to the rotation of the earth on its axis. An additional motion represents the conical wobble of the earth's axis which is known as precession and which results in the movement of the position of the pole of the sky. For example, over the course of several thousand years what we now call the Pole Star will cease to mark the direction of the earth's axis in space, and Vega will gradually come to fill this role.

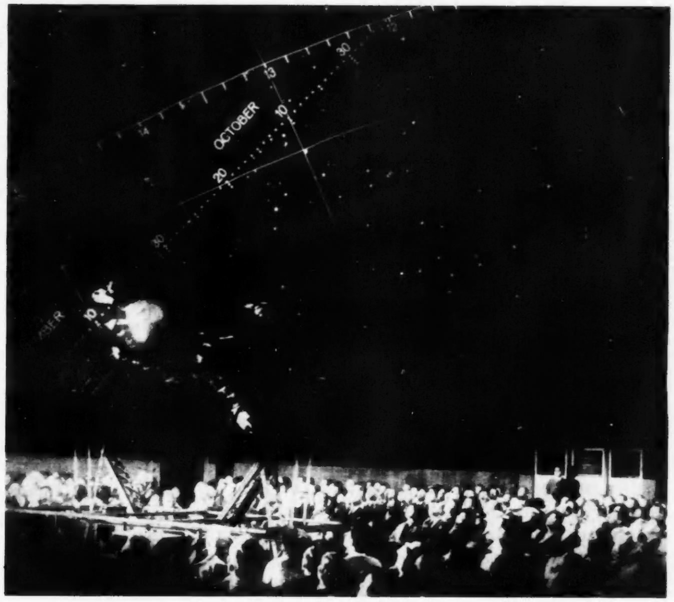
The projection of the planetary motions is achieved by the use of a scale working model of the solar system. Pins represent the positions of the earth and planets, and on these pins small lamps are mounted. It is so arranged that the light from these lamps is always projected on to the dome in a direction away from the pin representing the earth. In this way the spots representing the planets are always projected in the directions in which they would be seen from the earth.

The whole effect is remarkably lifelike and striking. The configuration of the planets at any time in the distant past or future can readily be reproduced, while changes occupying many thousands of years can be compressed into a few minutes.

In his letter, the Astronomer Royal suggested that an existing undamaged planetarium might be obtained from Germany as a reparations item. It is possible to criticise this suggestion, on the general ground that, after the last war, the exaction of reparations in kind from Germany had quite striking adverse effects on our own manufactures. In this particular case the argument is strengthened. The optical industry in Britain between the two wars fought

manfully to overtake the lead in reputation which the corresponding German industry had established. In the end a good deal of the difference lay in reputation and not in fact, but one of the effects was that foreign buyers tended to go to Germany or the U.S.A. in preference to this country. There is little doubt that a planetarium could be manufactured in Britain, and, although the first effort might well be inferior to the German article, the actual manufacture would give British industry invaluable experience which would represent a valuable future asset even though the demand for planetaria in Britain would certainly be limited to one or two.

As to the establishment of a planetarium in Britain, there can be no doubt of its desirability. Astronomy is a science in the curious position of having a great popular interest coupled with an *obvious* practical value that is rather small and, regrettably, decreasing. Whereas physics in general can rely on the support of industry, astronomy is something of a Cinderella whose financial position, never strong, is now so weak as seriously to interfere with progress. An appeal to the general public by such a method as the planetarium offers could, if the income it brought in were put back into the science, do much to restore to health a department of science in which, until recently, Britain was pre-eminent.



Most European countries have at least one planetarium; America has a considerable number. Interior of the Hayden Planetarium, New York.

To do this properly would mean something rather more than a planetarium. Three or four visits at most by any one person exhaust most of the possibilities of entertainment and instruction which are immediately apparent, and for those who wish to go further, demonstrations, observations and lectures of a different kind are required. Sir Harold Spencer Jones congratulated Sir A. P. Herbert on his felicity of expression in describing the planetarium as a 'Star House'. That name might well be adopted for it is vivid and pleasing to the ear; it also carries with it a recognition of the fact that a planetarium alone is not quite sufficient. Perhaps, as the Astronomer Royal suggested, the Science Museum is the best site for a Star House. It needs to be set up so that it will stand comparison with the magnificent features of public exposition which characterised some departments of the Science Museum, while at the same time the whole project should be invested with something of the spirit that inspired the originators of Paris's Palace of Discovery.

The Film in Schools

THE use of film in education is a subject engaging the interest of a growing number of educationalists, scientists, and film-makers. This interest is stimulated both by the needs of a rapidly expanding educational system and by the advances in the production and utilisation of 'non-theatrical' films which have taken place during the war years.

Film as a teaching aid has a special interest for scientists. It has been developed by scientific workers, and it must depend upon their researches for its continued improvement. For the exposition of scientific ideas and processes it is a particularly effective medium, being capable of exploring a wide variety of phenomena and of expressing abstract conceptions in visual terms.

The article in the February issue of DISCOVERY on 'The Scientific Interests of Children' indicated the enormous scope which the camera could have in the scientific education of children. In one of the subjects in which children show the greatest interest, biology, there already exists a considerable body of film experience and achievement, of which the best examples are the Gaumont British series 'Secrets of Life'. During the war the armed forces have contributed a great deal to educational film technique by the production of a large number of films for teaching the technology of various subjects, many of them, like radar, of a complex kind.

There can be no doubt that film as an educational medium has a distinctive and important role to play. To say that films are yet little used in education seems an understatement when one finds that the 30,000 teaching establishments of England and Wales had, in 1940, only 1700 projectors. The number of suitable teaching films appears to be well under 1000.

There exists a number of problems which will have to be solved if its full potentialities are to be realised. In the past the practical difficulties of showing films have discouraged many teachers, while others have been defeated by the lack of suitable films. On the other hand, one problem has been the bewildering number of libraries and the variety of catalogues. It has been extremely difficult for the teacher to have any clear idea of the suitability of the films available, or to know the simplest way of obtaining them.

In its latest broadsheet, 'The Film in Schools', published on behalf of the Arts Enquiry, P.E.P., puts forward a series of proposals designed to overcome these and other problems associated with the use of the film in the education of children. To raise the output of films of undisputed educational merit, and to ensure the production of the widest possible range of films, this broadsheet recommends the sponsorship of film production by the Ministry of Education. This "would for the first time enable the small independent production units to concentrate on teaching films, without in any way preventing the larger production companies from coming within a scheme of sponsorship or from making, selling, and hiring films on their own account".

This work would be greatly assisted if the Ministry of Education has its own Visual Education Council. The broadsheet proposes that such a Council should be set up, composed of practising teachers and educational administrators qualified to estimate the needs of the profession. In consultation with the Government this Council would have the responsibility of proposing a programme of film production and of suggesting methods of treatment and the choice of subject experts.

Assistance must be given to teachers in the selection and booking of films and this can best be done if the existing facilities are co-ordinated and expanded. Immediate steps should include the compilation of a national critical catalogue and the increasing of grants to bodies which appraise and catalogue educational films. It would also be a great help to the teacher if film libraries were built up by Local Education Authorities.

Finally, to overcome the acute shortage of projectors and the confusing variety at present in use, it is suggested that equipment should be standardised, so making mass production of projectors possible. These could then be bought in bulk by the Ministry of Education and resold to education authorities.

Dealing as it does primarily with the use of the film in schools, the P.E.P. publication nonetheless raises sharply the whole problem of the social role of the film. The full Arts Enquiry film report, which is to be published shortly, will undoubtedly stimulate still wider public discussion of this problem and will help to provide a basis for the formulation of more definite public demands.

House Warming

MANY of the visitors we have welcomed to our shores in the recent troubled years have commented on the warmth of our hospitality and the chilliness of the homes in which we tender it. Such comment may have stimulated the committee which has produced the report on Heating and Ventilation of Dwellings (Stationery Office, 2*s*. 6*d*.) which forms No. 19 in the Ministry of Works' series of Post-War Building Studies. The greater part of the volume is not concerned with research, but is an evaluation, by the committee of experts over which presided Sir Alfred Egerton, of existing knowledge. It makes some recommendations for post-war building practice and for research in this field.

The report—its topicality was heightened by the fuel shortage—makes no pretence to novelty, yet much of its contents will be new to most people. It is probably not generally known, for example, that to heat a room with ordinary walls—brickwork finished with plaster—for a mere two hours daily requires 25-35% of the heat needed to heat it continuously. This percentage can be roughly halved by lining the walls with an insulating material of low thermal capacity, such as wood panelling, fibreboard or cork tiles. The proportional saving obtained with such linings increases rapidly as the total time during which the room is heated *decreases*—a consideration that should be important when deciding how the various rooms of houses are to be 'decorated'. The basic concept put forward is the desirability of 'background heating' in homes, with 'topping-up' to bring the temperature to the desired level in a room in use. Due warning is expressed of the fact that it is possible to have too much comfort, resulting in the body losing its power of quick adaptation.

A serious attempt is made to give criteria of 'stuffiness' and 'freshness'—the draught is discussed in terms of air velocity and temperature gradient. Many facts that are otherwise difficult to obtain are here brought together. There is an excellent appendix on atmospheric pollution, which is estimated to cost us £1 per head per year. (Domestic users of coal are by far the most guilty, putting 1,290,000 tons of smoke and 140,000 tons of ash into the atmosphere each year—against 400,000 tons of smoke from the railways, 700,000 tons from industry in general, and only 10,000 tons each from gas works and power stations, whose record in this respect is very good.)

Not the least interesting part of the volume is the appendix reporting the results of a 'Heating of Dwellings Inquiry' carried out by Wartime Social Survey. This *is* a piece of original research, and furthermore it is one of the very few examples of the social survey method to be published *in extenso* in this country. It is concerned with the habits, likes and dislikes of working-class housewives.

One section of the Survey's report analyses in various ways the preferences of people for different methods of cooking, the reasons given for those preferences and the various factors that influence them. It concludes, for instance, that "Income level appears to affect preference for two reasons; electric cooking is believed to be more expensive, whereas coal cooking, which in many cases is combined with space heating, is regarded as being less expensive, also the poorer housewives are more conservative than those in the higher income group." The analysis also "illustrates the greater conservation of age" in the four age-groups—under 30, 30-40, 40-50, and over 50—those preferring coal numbered 12%, 16%, 21% and 29%, respectively, of the total; those preferring electricity, 42% 39% 32% 22%; while the percentages preferring gas were between 44 and 49 for all age-groups. That people are only too often forced to cook by methods they dislike is shown by the fact that, while 3% of the total used oil as fuel, only 1 out of 4808 (or about 0·2%) preferred it. Of those who expressed a preference for coal, 89% had experience of its use, and for gas the figure was 85%, whereas of those preferring electricity only 49% had actually used it.

This example serves to show the valuable social data that can be provided by the sample-survey method.

This remarkably extensive survey exhibits the literary disjointedness of the typical committee report, and gives the wide coverage expected from co-ordination of highly

skilled specialists. One might cavil at the rather brief discussion of foreign practice in domestic heating it if were not that a sub-committee is working at a report on district heating. An exceptionally valuable pamphlet for architects, builders, fuel technologists and ordinary citizens, it might well have been published in a less flimsy binding.

The Fuel and Power and Advisory Council of the Ministry of Fuel has just published a report entitled 'Domestic Fuel Policy' (Cmd. 6762). This may be regarded as an attempt to indicate how the data of the Heating and Ventilation report can best be applied in practice.

Science is a Human Affair

ARTHUR KOESTLER's postscript on March 17 nettled one of our readers so much that he wrote us a letter about it. He was annoyed by Koestler's attack on Professor Bernal, but we do not intend to enter into that argument; the quarrel between Koestler and *Modern Quarterly* that gave rise to the remarks about Bernal's philosophy is a private political quarrel. (Anyone who wants to look into it should read the two latest numbers of *Modern Quarterly* and *The Listener* for March 21 and April 4.)

This letter from a layman contained another point, however, which scientists often overlook. "Some small effort is now being made in various quarters to popularise the branches of general science," he writes. "But this is not enough. It is also necessary to popularise the scientist *per se* and to ensure that he be prescribed his rightful place in the esteem of his fellow men. The cold, microscope-gazing idea must be refuted for all time."

There are, of course, scientists who say they don't give a cuss about the esteem of their fellow men. But in view of the fact that ultimately they are entirely reliant upon those fellow men to provide the money needed for research that attitude always seems unreal and unconvincing.

On the other hand, scientists with social consciences, while they understand that there is a need for 'public relations', are liable to become so engrossed in their studies of the social relations of science that 'public relations' get put on one side. It is quite certain that the publicity of science would be far in advance of its present state if there was less lip service and more practice of what is preached about the 'public relations' of science. Insufficient practical attention to this aspect enables those humanists who are hostile to science to hammer away, with some prospect of success, at the wedge with which they seek to divide scientists and the general public. Relative to those humanists, the publicists of science are placed at a disadvantage because it is not considered *the thing* to infuse warmth, colour, drama and humour into their work. Dr. A. S. C. Lawrence in his Royal Society of Arts lecture on the future of the Scientific Film called attention to a deficiency in the recent and, as a whole, meritorious film about penicillin: it failed to put over the dramatic theme of the drug's discovery, manufacture and special value. That is true of a great deal of popular science. We believe Dr. Lawrence was well justified in saying too that the Victorians had a better sense of the drama of science, invention and industry than we have today. We support his view that there should be films, in the spirit of *First of the Few* and the Edison films, about scientists and engineers—about men like Watt and Perkin.

Science is a human achievement. The public does need to know something of the human background of scientific discovery, something of the jobs scientists do and the conditions under which they work. A novel like *The Small Backroom* or *Martin Arrowsmith* may in the long run be worth a hundred straight articles because it helps to kill the cold, microscope-gazing idea.

It would also be worthwhile for somebody to collect together in a book scattered descriptive essays dealing with scientific work in a similar way. Haldane has written several—in a London evening paper only a few weeks back there was one describing his war work in connexion with escape from submarines. In *France Libre* we recently found another instance, a delightfully written account of the everyday routine of astronomical observation.

It is because we consider this sort of writing valuable that we publish elsewhere in this issue Mr. Waldram's 'Atmosphere—May 1941'. The author, who is a physicist in the General Electric Company's laboratories at Wembley, wrote it originally for the entertainment of himself and his colleagues and with no intention of publication. (Some readers may reasonably ask where the scientific details of this particular experiment can be found; the reference is: the Transactions of the Illuminating Society, 1945, Vol. 10, No. 8, p. 147).

Edible Fat from Coal

MANY and wonderful were the stories about German ersatz. Jokes like the one about Hitler saying to Mussolini, "I'll teach you how to make butter from coal if you'll show me how to knit a pullover out of macaroni" helped us to forget the part that German successes in the hunt for substitutes played in the design of military strategy, and also to overlook the fact that very often something equally ingenious was being contrived here. (Most people can be absolved for the latter oversight for it long ago became British official tradition for stories about British achievements to be released at the thirteenth hour, after some other country had received all the credit for them.)

Britain never made butter from coal—nor, of course, did Germany—but the Fuel Research Station at Greenwich did succeed in synthesising fatty acids by adapting the Fischer-Tropsch process. Edible fats have been prepared from the combination of these acids with glycerine, though no attempt has so far been made to assess their nutritive value.

It is the considered opinion of the Fuel Research Station that the Fischer-Tropsch process could not be operated economically in this country at the present time, and as a source of synthetic fats it is very inefficient. The Germans, driven by reasons of blockade, to try out the process of the big scale found that it takes 60-70 tons of coal to make a ton of synthetic fat or soap.

The essence of the Fischer-Tropsch process is the conversion of coal or coke into a mixture of carbon monoxide and hydrogen; the gas mixture is then cleaned of sulphur compounds and passed over a catalyst at about 200° C. and at pressures of 1-10 atmospheres. With these gases as the starting-point it is possible to synthesise petrol, diesel oil, other high-grade lubricants, and waxes, and fatty acids of the type required for the preparation of soap and edible fats can be turned out.

In this article Professor Zeuner of the London University Institute of Archaeology deals with the measurement of geological time. In the near future we shall be publishing a complementary article by him dealing with the chronological aspects of evolution and entitled "Time and the Biologist".

Time and the Geologist

THE METHODS OF GEOCHRONOLOGY

Professor F. E. ZEUNER, D.Sc.

'ABSOLUTE' DATES, that is dates in years, and even months and days, are an essential prerequisite of the student of mankind's past. So far as the past is covered by written records, the working out of the correct sequence of events and their dating is largely a matter of literary research and the science engaged in it is called history. Where written records fail, prehistory begins. Since prehistoric chronology has no human calendar in years to rely upon, it has been necessary to develop for dating purposes a so-called *relative* chronology. This is a chronology in terms of development of some kind of human artefact, such as metal tools, pottery types, flint implements, which are known to have changed in the course of time, though the actual duration of the periods involved, and the rate of change, are not known. Prehistorians have, of course, never lost sight of the desirability of converting the *relative* chronology into an *absolute* one, and they have been eager to make use of any absolute time-scales afforded by extraneous evidence. Such time-scales are for the most part based on climatic fluctuations which occurred in the past and can be dated, more or less tentatively, by means of astronomical cycles, such as the year, the sunspot cycle, or the perturbations of the earth's orbit.

In biology, the situation is vastly different. One would expect that research concerned with the evolution of life should cry out for some sort of time-scale by which to measure the rate of evolution. Naturally, it would have have to be one with a much longer range than those applied to prehistory, one measuring the duration of geological epochs and periods. The need for time-scales of the geological past was still recognised by biologists in the second half of the last century, as for instance by A. R. Wallace, who, in his *Island Life*, devoted a whole chapter to the measurement of geological time. But, following the development of modern genetic research, the belief has grown that the time-factor plays but a small part in evolution. This neglect of the chronological aspects of evolution is, fortunately, not universal. Several outstanding biologists of today, notably Professor J. B. S. Haldane, have paid attention to it.

Palaeontologists, on the other hand, with their geological training, have always been greatly interested in problems of chronology, though it is mainly relative chronology that has occupied their minds. This is so in spite of the fact that reasonably reliable time-scales have been available for some time, which would enable palaeontologists to determine the rate of evolution of a good many lineages (pedigrees of species). Again, there are noteworthy exceptions. Among the workers who have considered the bearing of absolute time on evolution is Dr. G. G. Simpson of the American Museum of Natural History.

It might, of course, be argued that the real trouble lies in the time-scales available, that they are unreliable and sometimes just guesses. This is true only to a certain degree. Geochronologists have been busy sorting out the more reliable methods of obtaining time-scales and discarding those based on guess-work and unsatisfactory methods. This work has made great progress in recent years so that by now several methods are available which provide dates in years for events in human prehistory, the Ice Age, the evolution of life on earth and the earliest stages of the earth's history. Many of these dates are very approximate and subject to correction, but we can feel confident that they are of the right order of magnitude, since a fair number, obtained independently, have been found to be consistent with each other.

There are four geochronological methods in use at the present, each covering a different portion of the time-scale:

(1) *Tree-ring analysis*, relying on the cycles of the year and the sunspots; covering historic and prehistoric phases mainly in the south-western U.S.A.; not likely to be extended over more than 3,000 years before the present.

(2) *Varve analysis*, relying on the cycles of the year, the sunspots and the precession of the equinoxes; covering the time from the end of the Old Stone Age to the Iron Age and extending over the last 15,000 years.

(3) *Astronomical method*, relying on changes in solar radiation caused by the cycles of the perturbations of the earth's orbit; covering the Ice Age and the Old Stone Age, and extending back to about one million years before the present.

(4) *Radioactivity method*, relying on the rates of decomposition of radioactive minerals; covering all geological periods before the appearance of man and extending over at least 1,500 million years.

Tree-ring Analysis

Tree-ring analysis was invented by the astronomer of Arizona University, Professor A. E. Douglass. Everybody is familiar with the growth-rings formed by trees which can be seen in any stump or section of wood. These rings, built by the tree beneath the bark, begin with large cells in spring and end with small, thick-walled cells in summer. Each ring, therefore, represents one year of the tree's life, and the age of the tree can be calculated from the number of rings. If the cutting date is known, the year of germination can be found. Now, Professor Douglass noticed that in the semi-arid country of Arizona the width of the

rings depends on, among other factors, the amount of rainfall in the particular year, droughts resulting in very narrow rings, or no ring may be formed at all. In other districts, where rainfall is more regular and where trees have permanent access to water, these differences in ring-width are less conspicuous. Droughts affect, of course, most trees of a district simultaneously, irrespective of their individual ages. Records of droughts are thus left in the form of narrow rings, which, in trees which were old at the time, lie near the periphery; in those which were young at the time, near the centre. If several such drought rings occur and can be identified in two trees, it is clear that the two together provide a stretch of time-record which covers the lifetimes of both. Suppose the cutting date of the younger tree is known, you can get the time-scale counting back to the germination year of the older tree. But trees are used as timber in buildings. You can count and measure the rings in timber just as well. If you know its origin, local in most cases, and you have studied a large number of trees, young and old, living and dead, and the timber as well, you may be able to obtain a continuous time-scale after the manner illustrated in Fig. 1. Cutting dates for pieces of timber can thus be determined which lie several hundred

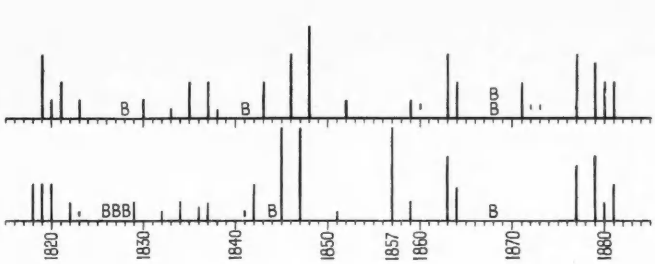


FIG. 2.—Two tree-ring plots constructed by Professor Douglass. Normal rings are not marked but merely counted. A vertical line indicates a *narrow* ring; the longer the line, the narrower the ring. *B* and *BB* indicate unusually broad rings. The two plots shown are for two Arizona pine trees from the same locality. No ring was formed in the year 1857 by the tree represented in the upper plot; this accounts for the fact that the ring plots prior to 1857 are out of step by one year. (Based on Glock; reproduced from *Dating the Past*.)

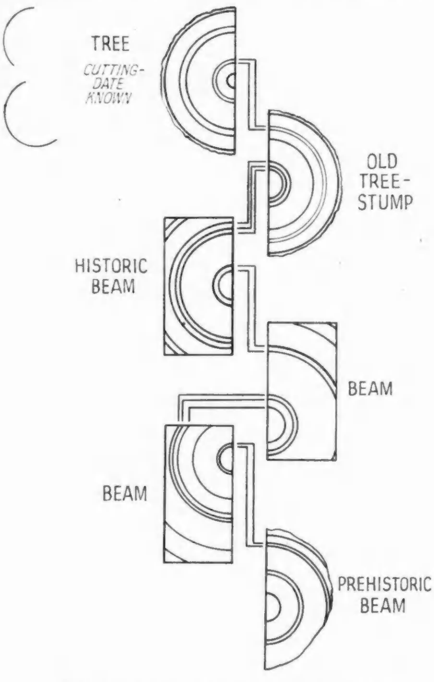


FIG. 1.—Schematic drawings of sections of trees and beams from historic and prehistoric buildings, illustrating how peculiar sets of tree-rings can be recognised in different trees and used for cross-dating. (From *Dating the Past*, Methuen, 1946.)

years back. Professor Douglass and his collaborators have in the course of many years managed to obtain a continuous time-scale of more than one thousand years. Its early part depends on timber from the famous pueblos—Indian village-houses—of Arizona, and it was possible to date with their aid not only the villages themselves, but also the succession of prehistoric cultures, the remains of which were found in the houses. The dates for the cultures are likely to apply in a much wider area than that of the pueblos, as the villages are called, so that the prehistoric time-scale obtained is of more than local significance. In using the term *prehistoric* here it must be remembered that in North America everything before A.D. 1492 is prehistory. It was found that the various 'pueblo' periods begin about A.D. 750, and that the preceding 'basket maker' culture covers at least the time from A.D. 400 to A.D. 750.

There are, of course, many difficulties in the method which cannot be discussed here. As an example, Fig. 2 shows how the absence of one ring in a tree can be discovered by comparing it with the record of another tree of about the same time. Furthermore, since the method depends largely on timber it can be applied only where timber is preserved in abundance. Sometimes years have been spent in searching for a piece that would bridge a gap in the time-scale. It was the continuity of habitation in the pueblo district that made the time-scale possible. Mere counting back has been carried further by Professor Ellsworth Huntington of Yale, and Dr. Antevs of varve fame, by studying the 'Big Trees' or giant redwoods (*Sequoia*), of California. In all, a record of the last three thousand years has been covered by tree-rings.

Then, tree-ring chronologies must depend on local material influenced by the weather in the neighbourhood of the locality. One cannot safely connect tree-ring records over long distances simply because the weather of two localities is likely to differ the more the farther apart they are.

Nevertheless attempts have been made to use the records of Californian *Sequoia* for dating prehistoric sites in Sweden and Norway. Unbiased workers will be reluctant in accepting such 'datings', but the reason why they have been attempted is rather interesting.

When investigating large numbers of ring-series,

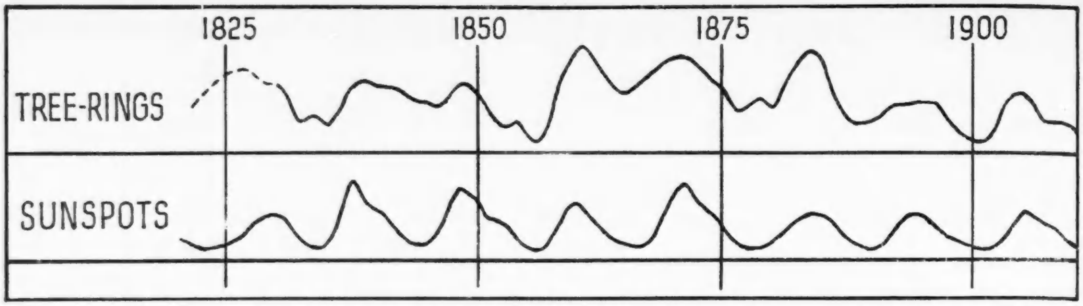


FIG. 3.—A curve obtained by plotting ring thickness for Scotch pine from Europe compared with the curve of sunspot activity. Note that both curves exhibit the same cycle of slightly over 11 years. (Based on Douglass and Glock).

Professor Douglass noticed that quite apart from the differences in ring thickness due to weather conditions, they contained other fluctuations in thickness which were of a periodical character. For these latter fluctuations, the most conspicuous cycle turned out to be one of a little over eleven years. In Fig. 3 this periodicity of tree-growth is compared with the cycles of the sunspots. The rhythms of tree-growth and of sunspots activity are indeed so similar that a causal connexion is suggested.

Of course not every tree shows the sunspot cycle in its rings, though it has been found in many from different parts of the world. Studies carried out in the London University Institute of Archaeology have shown that the eleven-years cycle is present in woods from the tropical rain-forest of West Africa. The moist-temperate climate of northern Europe also favours the reproduction of the eleven-years cycle in the tree-ring records. But Professor Douglass's tree from semi-arid Arizona exhibits *two* complete oscillations in every eleven-year period, instead of the single one found in damper climates. The interpretation of this curious phenomenon is by no means clear. That the eleven-years period of tree-growth *is* connected with the sunspot cycle is borne out by the fact that variations in the length of the latter are faithfully reproduced by the trees. In particular, the so-called dearth-period of ten years which sunspot activity displays occasionally has been found in tree-rings also. The investigation of cycles shown by the tree-rings has thus become an interesting subject by itself. It offers prospects for the ultimate extension of ring-countings to earlier periods than those hitherto tackled, provided certain types of cycles can be identified in the wood. But before this becomes possible much work remains to be done on the cycles themselves. Professor Douglass has contributed a great deal to the development of this subject which is called *cycle analysis* and for the fostering of which in all branches of science and economy a Foundation for the Study of Cycles has been established in the U.S.A.

FIG. 4.—Varves, annual layers deposited by the melt-water from ice of the Penultimate Glaciation, at Opava, Czechoslovakia.

Varve Analysis

Varve Analysis. As long ago as 1878, Baron Gerard de Geer, who later became the first Professor of Geochronology in the University of Stockholm, conceived the idea of counting the annual deposits laid down by the receding glaciers of the Last Glaciation in Scandinavia. Meltwater from these glaciers accumulated in many places in shallow lakes. In summer, when the flow of meltwater was plentiful, more sandy detritus was introduced into the lakes, whilst in autumn, when the water supply froze up, the mud suspended in the water gradually settled down. Thus a thin double layer was formed each year, coarse below and fine above, and usually ranging in thickness from a few centimetres to as much as forty centimetres (Fig. 4). For such regular, annual layers the Swedish word *varve* (which simply means *layer*) is used, hence the term *varve analysis* for the study of deposits of this kind.

The basic idea underlying varve chronology is much the same as that of tree-ring chronology. Years with hot summers cause exceptional melting and produce thicker varves than normal years, while cool summers make the corresponding varves unusually thin. It is claimed that such varves can often be recognised not only in the same lake but in neighbouring lakes, especially if several distinctive varves appear in association. The identification of such series of exceptional varves was in the early days based almost exclusively on their relative thicknesses; now other characters such as composition, colour and texture are used also. It is obvious that cross-dating of varve series from neighbouring lakes, the ages of which overlap to some extent, can be carried out in much the same way as shown for trees in Fig. 1.

Since the skeleton of a relative chronology is provided by the 'garlands' of terminal moraines left behind by the receding ice, varve series are worth studying even where they do not overlap in time. The gaps mean, of course, that stretches of the time-scale are still missing and that the sum total of varves counted so far gives us merely the *minimum* age of the oldest deposit.

Fortunately, central Sweden (Stockholm area) and southern Finland have provided many sections of varved clays, which can be cross-dated. They are connected with the great belt of moraines which runs from Sweden (where it is called the Central Swedish Moraine) across the Gulf of Bothnia to Finland (where it is called the Salpausselkä Moraine). As the result of the withdrawal of the ice from this moraine, salt water entered the Baltic, and increased flocculation of suspended mud caused the character of the varves to change. This event provides a good time-mark. Corresponding event on both sides of the Gulf of Bothnia have been dated by independent work; de Geer and his pupils worked in Sweden, while Professor Sauramo of Helsinki University is the foremost varve specialist in Finland.

In addition to this event, the sudden emptying of a lake in the Scandinavian mountains provided an unusually thick varve—the Ragunda drainage varve—and this was adopted by de Geer as zero point for his time-scale. Around these two events a varve sequence was built up by patient counting which developed into a chronological record of the phenomena connected with the retreat of the ice-margin and covering several thousand years. The portion of time-scale obtained in this manner enabled workers to date in years (relative to the zero point just mentioned) many events in the history of the Baltic Sea, climatic phases of the Postglacial, and the development of the Mesolithic and Neolithic cultures of early man. But for a long time the link with the present day was missing. Eventually Almar Lidén, a Swedish geologist, succeeded in collecting, along the Angerman River, varve sections sufficient to cover the gap between the Ragunda series and the present day. Thus it was found that the division of the Scandinavian ice-sheet into two residual caps, with which is connected the formation of de Geer's zero varve, took place about 6800 B.C., and that the halt of the ice at the Central Swedish Moraine occurred about 8000 B.C. (Fig. 5.)

These dates appear to be reasonably reliable. Professor de Geer has, however, attempted to extend the varve chronology back into the earlier past by undertaking countings in southern Sweden and Denmark. The results obtained here have still to be regarded as highly conjectural, the Danish workers, notably Dr. Sigurd Hansen of the Danish Geological Survey, maintain that they are based on a mistaken interpretation of a lamination in the deposits which is *not* of an annual nature. It is necessary therefore to suspend judgment on the earlier part of de Geer's varve chronology which is thought to go back to about 13,000 B.C. This is of some importance since this earlier part has been used by de Geer to achieve what he calls *teleconnection*, i.e. correlation across the ocean, with certain varve sections in North America which had been studied by the Swedish-American geologist, Dr. Ernst Antevs. These sections are sound enough, but if the early part of the Swedish time-scale breaks down an important element in the American varve chronology (which is still in its infancy) breaks down.

The later and more reliable portion of the varve time-scale has provided a wealth of information, in the first instance on geological events connected with the ice retreat and the development of the Baltic Sea. The climatic phases of the Postglacial, revealed mainly by the analysis of the contents of tree-pollen found in peat, also have been dated by varve chronology, though only indirectly. Varve sections are usually sterile, since at the time and in the place of their formation the climate was unfavourable for much plant-growth, but after the ice had disappeared and varves ceased to form vegetation established itself, and layers of rich mud in organic matter and of peat were deposited on top of the varved clays in some places. Where the varves have been dated they provide a maximum limit to the age of the oldest of the organic deposits.

Similarly, beach deposits of the Baltic have been linked with varve sections. Beach deposits, chiefly platforms and ridges formed by the waves, are by nature horizontal and their height corresponds closely to that of the sea-level of the time. Beaches of this kind have been associated with varve sections, mainly for that part of the history of the Baltic when the receding ice formed part of the 'shore-line'. A fortunate combination of factors has made it possible to study the succession of these beaches in detail. While under the weight of the ice-cap of the Last Glaciation, Scandinavia, or rather the part of the earth's crust around Scandinavia, had been depressed by this weight, and as melting proceeded and the ice vanished the release from the weight made large portions of Scandinavia rise, with a certain time-lag. This rise, which is still continuing, has raised the earlier shore-lines to considerable heights (to over 250 metres), and the later ones less so. Thus, the beaches are now found at heights above the present sea-level which vary with their age. As one moves away from the centre of this uplift, which lies on the west coast of the Gulf of Bothnia, the movement is less intense, and on the German and Danish coasts of the Baltic a certain amount of compensatory subsidence appears to have taken place. These movements, and with them the ancient shore-lines, have been dated by means of varves. The main work was done by Professor Sauramo and his Finnish colleagues. The major stages of the Baltic, their relations to the retreating ice-sheet, and their approximate dates are shown in Fig. 5.

Laborious work on the beaches and peat deposits has given us a fairly detailed history of the climate since the ice of the Last Glaciation began to retreat from the Baltic and Scandinavia. This history of the climate serves as a *relative* chronology in dating the cultures of early man. Many finds, including settlements, have been made in peats and on beaches, and if their relative age is determined carefully, either by pollen-analysis or determination of the beach-level, the time-scale based on the varves can be applied to prehistoric objects also. It was thus found that the famous Maglemose culture of Scandinavia flourished between 7500 and 5000 B.C., and that the *kitchen-middens* of Denmark accumulated round about 4000 B.C.

Astronomical Method

Astronomical Method. The geological epoch of the Ice Age or Pleistocene, and the prehistoric cultures of the Old Stone Age or Palaeolithic, are covered by a time-scale

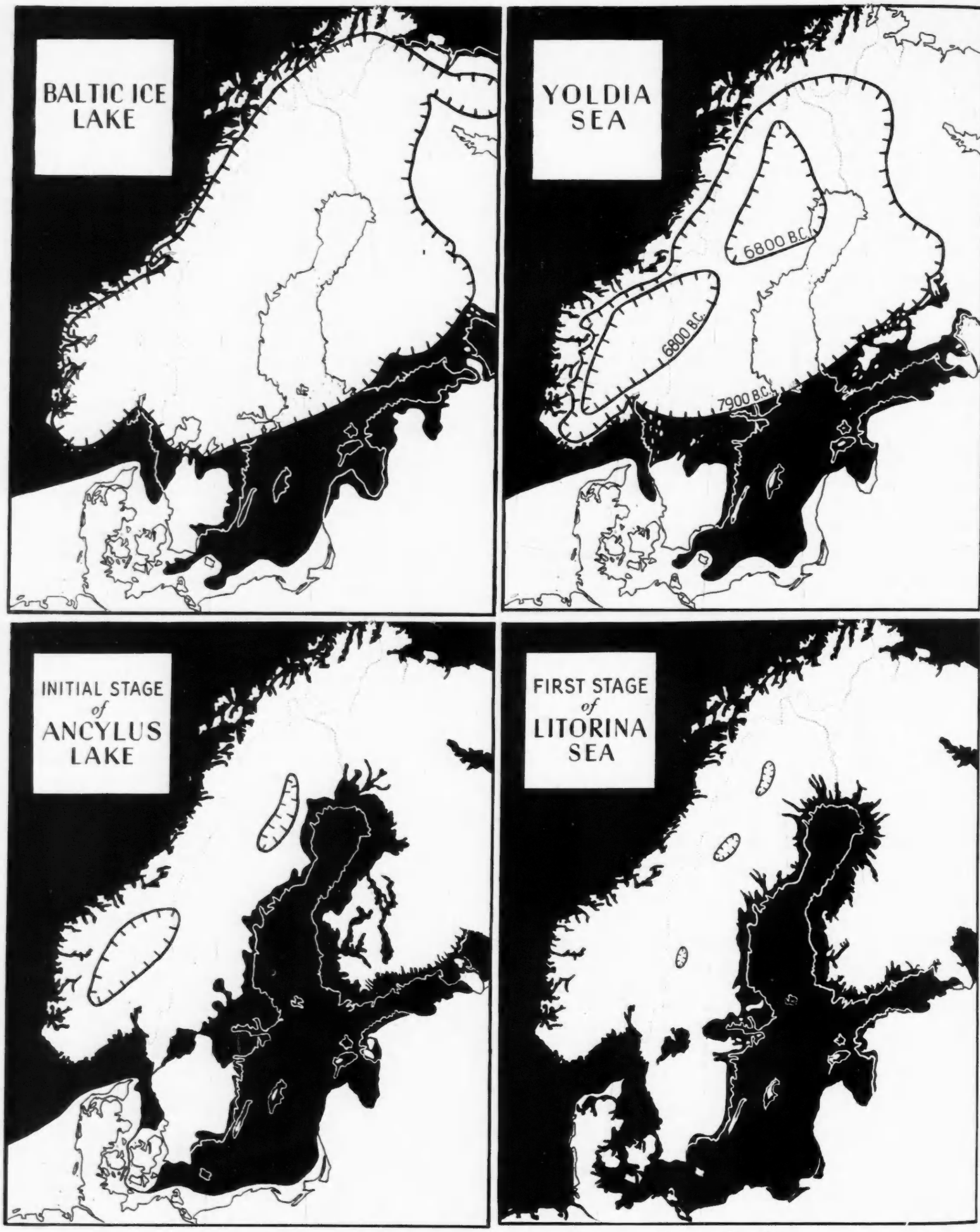


FIG. 5.—Four stages in the development of the Baltic Sea and the meeting of the Scandinavian ice-sheet of the Last Glaciation. (*A*) *Baltic Ice Lake* stage, about 8800 B.C.; the time of early Mesolithic Man. (*B*) *Yoldia Sea* stage, about 7900 B.C. Recession of the ice by 6800 B.C. is also indicated. The ice margin of 7900 B.C. is known as the Central Swedish Moraine. Time of Late Mesolithic Man. (*C*) *Ancylus Lake* stage, about 6500 B.C. Climate was then approaching modern conditions, but was more continental. Lake Mesolithic culture continuing. (*D*) *Litorina Sea* stage, about 5000 B.C. Neolithic Man has appeared (kitchen-midden culture). (From *Dating the Past*.)

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based on astronomical computations. This method of dating is the oldest of the four geochronological methods, at least so far as the conception of the idea goes. The first attempt to use it was made by the French astronomer Adhémar in 1842. Improvements were introduced by the British astronomers Croll in 1863 and Ball in 1890. The mathematical basis of the method was greatly developed by the Frenchman, Leverrier, the American, Stockwell, and by the German, Pilgrim, but it was not until Professor Milutin Milankovitch of Belgrade University recalculated and tabulated the numerical data that the astronomical method of dating the Ice Age and the Palaeolithic became a practical proposition. This happened between 1913 and 1938.

The astronomical method makes the perturbations which the earth's orbit suffers (owing to the mutual attraction of the planets) responsible for changes in the amount of radiation received by the earth from the sun. Compared with the size of the orbits these perturbations are merely slight wobbles, yet they are sufficient to modify terrestrial climate. The chief perturbations are the changes in the obliquity of the ecliptic, the eccentricity of the orbit, and the precession of the equinoxes; space does not allow to explain them here though the effects of changes in the obliquity of the ecliptic may be gathered from Fig. 6. The effects of the perturbations are quite insignificant if annual totals of radiation are considered, and this is why in the past most authors neglected them altogether. But if the distribution of radiation over the seasons is calculated, very considerable effects are discovered. In our latitudes, for instance, the perturbations produce alternating periods with an equable, 'oceanic' climate with mild winters and cool summers, and periods of 'continental' climate, with cold

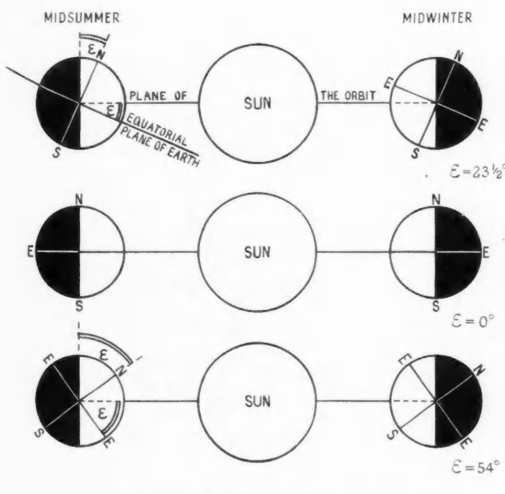


FIG. 6.—The influence of the obliquity of the ecliptic on the climate of the seasons. *Top*—Obliquity at its present value (23½°). *Centre*—imaginary case of obliquity being zero; No seasons would exist, but geographical zonation of climate could be intense. *Bottom*—Imaginary obliquity of 54°; seasons would be very marked, with hot summers and cold winters, but geographical zonation would be reduced to a minimum. (From Zeuner, *Pleistocene Period*, 1945.)

FIG. 7.—Loess deposits at Bon-Secours, near Rouen. Note that loess—wind-blown dust—is not stratified. The horizon which divides the section in the middle corresponds to a period when no loess was formed, i.e. to a climatic oscillation. Loess sections play a great part in the relative chronology of the Ice Age.

winters and hot summers. The cycles of the perturbations being of the order of 20-90 thousand years, the fluctuations of the climate have a periodicity of the order of several tens of thousands of years. They can be calculated with great accuracy for about 600 thousand years back into the past, but beyond this limit accuracy decreases rapidly owing to the interference of other factors. It is unlikely that any chronology based on the perturbations and continuous with the present time can be extended beyond one million years from today.

There are several reasons why all authors before Pilgrim failed in their attempts to use the perturbations. Firstly, they tended to consider the effects of the three major perturbations separately. Furthermore, they did not undertake to calculate the effects numerically, and lastly they neglected to a varying degree the differences in the effects on seasons (instead of the whole year) and on different geographical latitudes (instead of the whole surface of the earth). These factors were duly considered by Professor Milankovitch, and the numerical effects calculated in calories separately for summer and winter, and for every tenth degree of latitude. At the moment opinions still differ as to the changes in temperature brought about by fluctuations of solar radiation on the surface of the earth. That they were intense enough to affect the climate is, however, proved by geological evidence.

Since 1909, when Penck and Brückner completed their famous work on the Alps in the Ice Age, in which four glaciations separated by three interglacials were established, many Pleistocene geologists have discovered further subdivisions. Terminal moraines and the fans of gravel that

FIG. 9.—A raised beach (ancient shore) at Hope's Nose, Torquay. (Photograph by the author.)

are washed out from them suggested to the Rev. B. Eberl of Günzburg, Bavaria, that the three earlier glaciations comprised two phases each, and the last, three. Professor Soergel of Freiburg found that the river terraces of central Germany suggested that there were nine major cold phases making up the four glaciations and some less intense cool phases which interrupted the interglacials. Furthermore, the loess (wind-blown dust), accumulated during the glacial phases, was known to be divided by a number of fossil soils, indicating mild climate, into a number of levels each corresponding to a period of cold climate (Fig. 7). From all this evidence, abundantly confirmed by the results of other workers, a peculiar type of climatic rhythm emerged for the Pleistocene, as shown in Fig. 8A. Moreover, Penck had made an attempt to estimate the duration of the Postglacial and the interglacials, from which a duration of 600,000 years may be deduced for the whole of the Ice Age. The Postglacial would have lasted some 20,000 years, and the middle interglacial, by far the longest, about 240,000 years. But these figures were based on extrapolation from the rate of weathering and sedimentation observed in Postglacial times and were, therefore, highly conjectural.

Now, the fluctuations of solar radiation produced by the perturbations can be represented in a diagram of the type shown in Fig. 8B. In it only the amount of radiation received during the *summer* half of the year is shown, the maxima and minima of summer radiation corresponding to minima and maxima of winter radiation. The 'winter curve' therefore looks much like the summer curve turned upside down and need not be shown. Each minimum of summer radiation indicates a period of cool summers and mild winters, and therefore of intensified glaciation, because of reduced melting in summer and increased snowfall in the mountains, where the glaciers originate. Since the minima on the variation diagram are dated, being the cumulative effects of the perturbations, theoretical dates are thus obtained for glacial phases. Provided the minima of summer radiation can be matched with glacial phases established on geological evidence, a time-scale is obtained for the climatic phases of the Ice-Age.

If one compares the radiation diagram, Fig. 8B, with the diagram illustrating the geological evidence for climatic phases, Fig. 8A, one cannot fail to notice that the former exhibits the same peculiar rhythm as is shown by the latter.

Moreover, the time-values suggested by the radiation diagram agree well with Penck's estimates: 600,000 years for the whole Pleistocene, 20,000 years for the Postglacial, 60,000 years for the Antepenultimate and Last Interglacials, and 240,000 years for the Penultimate or Great Interglacial. To regard this agreement as accidental would require an almost pathological dose of scepticism. The dates thus suggested by the radiation diagram, or rather diagrams and tables for different latitudes, cannot be regarded as very accurate since a number of factors must have caused a time lag of the climatic phenomena. Much time has been spent by a number of workers to assess this lag which appears to have been not more than 5,000 years in the case of the Last Glaciation.

The astronomical method of dating the climatic phases of the Pleistocene is capable of being applied to the Mediterranean pluvials (periods of increased rainfall) and also to the tropics and the southern hemisphere. But in

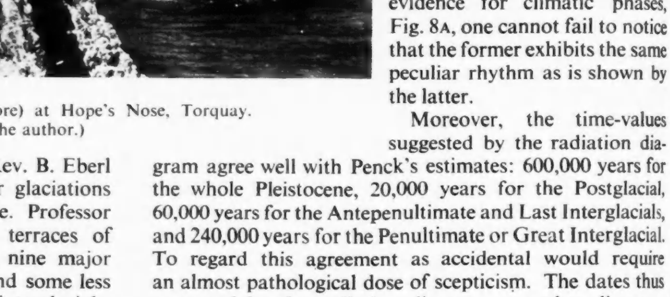


FIG. 8A (*top*).—The climatic curve of the Ice Age, combining evidence for four glaciations and their subdivisions, the relative lengths of the interglacials and Penck's estimates of their duration. FIG. 8B (*bottom*).—The curve of summer radiation as derived from the perturbations, for latitude 65°N. Note the resemblance of the curves and of the time intervals. The question marks indicate minor oscillations of doubtful position. The horizontal scale of Fig. 8B is in units of 50,000 years.

most parts of these regions geological evidence is still very sporadic. Research is being actively pursued, however, both from the geological and from the climatological angle. The most promising approach to a world-wide absolute chronology of the Pleistocene appears to be through the remains of ancient shore-lines. During the interglacials the water that during the glacial phases was locked up in the ice sheets returned to the oceans. High sea-levels therefore correspond to interglacial phases. Their traces are often well preserved and can be studied in detail (Fig. 9), and since the level of the sea may be taken as practically horizontal, the height of the ancient shore-lines must be the same in different parts of the world, provided they were not displaced by tectonic movements.

The resulting absolute chronology of the last 600,000 years of course requires much working out in detail, and corrections of certain dates at present regarded as sound are bound to be made. But even so it is already illuminating from many points of view. Not only does it help the geologist in obtaining figures for the rates of geological processes, but it also enables the archaeologist to date the phases of the Old Stone Age, and it suggests to the biologist means of grappling with the rate of evolution of species.

	Approximate Age	*Method*
Beginning of Pueblo civilisation in S.W. North America	A.D. 750	Tree-ring analysis
Division of melting Scandinavian ice-sheet into two parts	6800 B.C.	Varve counts
Formation of Central Swedish moraine	7900 B.C.	Varve counts
Maximum of 3rd phase of Last Glaciation	20-25,000 years ago	Astronomical method
1st phase of First Pleistocene Glaciation	590,000 years ago	Astronomical method
Beginning of Tertiary era	70 million years ago	Radioactivity (lead content)
Beginning of Mesozoic era	200 million years ago	Radioactivity (lead & helium)
Carboniferous	225-275 million years ago	Radioactivity (lead & helium)
Beginning of Palaeozoic Era	500 million years ago	Radioactivity (lead)
Late Precambrian	570 million years ago	Radioactivity (helium)
Middle Precambrian	1100 million years ago	Radioactivity (lead & helium)
Earliest rocks studied	1750 million years ago	Radioactivity (lead)
Mount Ayliff and Morden meteorites	6800 million years ago	Radioactivity (helium)

TABLE 1.—EXAMPLES OF DATES DETERMINED OR ESTIMATED BY MEANS OF TREE-RING ANALYSIS, VARVE COUNTS, ASTRONOMICAL METHOD, OR RADIOACTIVITY METHODS.

Radioactivity method. The three geochronological methods discussed so far cover only a relatively short period of time, but they give us comparatively detailed time-scales for the final episode of the earth's history during which the evolution of man took place. All the earlier epochs are covered by a method measuring time in millions of years. It relies on the disintegration of radioactive minerals.

Nowadays it is unnecessary to sacrifice much space for the description of radioactive disintegration. The chief families of radioactive elements used for age estimates are those of uranium, thorium and actinium. Several stages in the disintegration sequences involve the release of an α-particle (helium ion); the end product of the disintegrations is lead, varying in atomic weight according to the parent element. The accumulation of lead and the release of helium are functions of time and are unaffected by the temperatures and pressures prevailing in the rocks.

One million grams of uranium produce 1/7,600 gram of uranium-lead (Radium G, atomic weight 206) per year. If the amount of uranium-lead present in the specimen is Pb^u, and that of uranium, U, the time that was required to produce the amount of uranium-lead present in a sample —in other words, the age of the mineral—is $Pb^u/U \times 7{,}600$ million years. This is the type of formula which, with certain corrections, is used in the determination of the age of rocks containing radioactive minerals.

Measurement of the accumulated helium is the basis of a separate method, that can be used provided the rock is dense enough to retain the gas. Some helium, however, is always lost, and most results are accordingly unreliable. Magnetites, which have a very high helium retentivity, are exceptions and have in recent years yielded excellent results to the American geophysicists, Drs. Goodman and Hurley. Other workers on the helium method are Dr. Urry of Harvard and Professor Holmes of Edinburgh.

The greatest difficulty from the chronological point of view is to find rocks which satisfy two conditions. First, the rock must comply with certain technical requirements of the analysts (which cannot be discussed here), and secondly its stratigraphical (relative) age must be known. These conditions are not often satisfied simultaneously, since nearly all radioactive rocks are of igneous origin, while the stratigraphical scale relies almost entirely on the succession of sedimentary rocks. In view of these difficulties the dates obtained cannot yet be regarded as final,

but it is remarkable how large a number of age estimates are consistent with each other, so that they may be assumed to be not too far off the mark. These estimates provide, in the first instance, a rough idea of the date of certain events the stratigraphical age of which is known. Thus, pitchblende from Silesia dates the Lower Carboniferous at 269 million years ago, and pitchblende from Bohemia the Lower Permian at 220 million years ago. In the second instance, wherever a number of such 'point-dates' cover several sub-divisions of a geological epoch or period, approximate figures for the duration of that epoch may be obtained. The lower boundary of the Jurassic may be estimated, from a number of determinations covering late Triassic and early Jurassic volcanic rocks from eastern Canada, as being round about 150 million years ago. On the other hand, the Jurassic-Cretaceous boundary is indicated by some determinations which yielded for Lower Cretaceous rocks an age of slightly more than 100 million years, whilst later Jurassic rocks suggested ages of 123 and 103 million years. Making allowances for various sources of error, it thus seems likely that the Jurassic period lasted for something like 40 million years. Figures of this kind, approximate though they may be, are of great value in assessing the rates at which geological processes work, as well as the time-rates of biological evolution.

Radioactivity dating is a line of research most actively pursued in the United States, where a Committee on the Measurement of Geologic Time has been instituted by the National Research Council under the chairmanship of Professor A. C. Lane and including W. D. Urry, N. B. Keevil and others who form an effective team. In Britain, Professor Arthur Holmes of Edinburgh is the leading authority, whose book on *The Age of the Earth* is well known. Professor F. A. Paneth, working in Durham University, has made notable contributions to the determination of the ages of meteorites. Quite recently, a new line of radioactivity dating has been developed by Drs. C. S. Piggot and W. D. Urry in the Geophysical Laboratory of the Carnegie Institution, and by Professor Hans Petterson of Stockholm University. Though radioactive disintegration provides the means of measuring time, the principle of the method is vastly different from the traditional one, relying as it does on the gradual establishment of an equilibrium between various radioactive elements present in the sample. This new method has been designed to date cores of sediment brought up from the bottom of the ocean. It has a promising future since it covers the last few hundred thousand years only and thus affords a new way of dating Pleistocene marine deposits, a way which is independent of the astronomical method used in the dating of terrestrial deposits of Pleistocene age.

It must be frankly admitted that absolute chronology is still in its infancy. On the other hand, the methods employed are no longer of the 'pure guess' variety. For various portions of the time-scale, different methods have been developed which, ideally, should be fairly accurate, and it is probably only a matter of time before the numerous difficulties, which make the dates so far presented only partially reliable, will be conquered. The summary table, Table 1, contains some specimen dates; it must not be regarded as the final word in the matter, but merely as giving the stage in which geochronology is today.

PROFESSOR HAHN ON THE ATOMIC BOMB

THE *Neue Hamburger Press* of February 9 contains some interesting statements made by Professor Otto Hahn in an interview. Apparently there is a widespread rumour in Germany to the effect that the bomb was a German invention that the Allies took over. Professor Hahn, whose discovery of the phenomenon of nuclear fission before the war was the the starting point for the research effort which led to the development of atomic bombs, apparently felt it necessary to contradict this rumour. He points out the impossibility of the idea, "because I was captured by American troops on April 25, 1945, and the first atomic bomb fell on Japan on August 6, 1945. It would be impossible to set up production of atomic bombs in so short a time."

"The Americans were in advance of us by three years in the manufacture of the atomic bomb," continued Professor Hahn, "because they invested a vast amount of money in their research, whereas we were prevented by our slender means from producing an atomic bomb at all. When I made my discovery in 1939 I did not think of using atomic fission for purposes of war at all. During the war Professor Heisenberg, Professor Harteck and others worked on the exploitation of atomic energy for driving heat engines, that is to say simply as a source of energy. We succeeded in producing a radioactive element of atomic number 93, which, however, is not stable. We knew that an even heavier element of atomic weight 239 and atomic number 94 (plutonium) must exist but we did not succeed in producing this substance. On the basis of their advanced research the Americans succeeded in discovering plutonium and producing it industrially. Element 94 is the atomic bomb."

With regard to heavy water Professor Hahn said, "We believed that heavy water was necessary to 'split' the atom, but this is not so. The Americans proved that this can be achieved more simply and effectively with carbon."

Professor Harteck, who was with Professor Hahn at this interview, then gave a vivid description of the damage an atomic bomb can do, ending with the statement, "Even if someone succeeded in escaping the effects of pressure and heat, for instance in a concrete shelter, he would still face certain death. The radioactivity released causes new radioactive elements to be formed everywhere. Radioactive rays which cause fatal internal burns are then emitted by all objects for a certain time; by some for only a few seconds, and by others for hours or weeks depending on their composition. We can only hope there will never be a war fought with these terrific weapons."

At the time this report was published Professor Hahn was in Westphalia, together with Professor Laue, Professor Gerlach, Professor Weizsäcker and Professor Heisenberg. Hahn stated that he had been brought to Britain via Belgium and France, and that he and his colleagues had done no scientific work at all since the capitulation.

The technical papers read at the four-day radar convention organised by the Institution of Electrical Engineers are in the main outside the scope of a journal like DISCOVERY. There were, however, many points of general interest in the lecture of Sir Robert Watson-Watt in which he outlined the development of radiolocation in Britain. Below is a summary of that lecture. Readers will find it gives historical perspective to the radar articles we published in September and December 1945.

The Evolution of Radiolocation

Sir ROBERT WATSON-WATT, C.B., F.R.S.

I WOULD not embark on a survey of the British field without saying that it would be foolish to claim priority in the inception of radar for either Britain or America. The work done before the autumn of 1940 was completely independent in the two countries; it utilised the freely available stock of international scientific knowledge. The work done after the autumn of 1940 was completely interdependent. Since 1941 I have enjoyed no higher privilege than that of serving as one member of a United Nations team whose members seek no fine subdivision of the credit which is due to all.

Sir Robert Watson-Watt

The United Kingdom part of radiolocation by itself was a truly great engineering enterprise. It occupied approximately half of that 1% of our population which was directly engaged in developing, producing, installing, maintaining and operating radio equipment for war purposes. On the production side the expenditure on radio material, and the engineering services immediately related to that material, was approximately £1,000,000,000 during six years of war and a substantial fraction of that sum during the four years of active defensive preparation which followed the inception of radiolocation in February, 1935. Approximately half of this wartime expenditure and a much greater fraction of the immediate pre-war radio expenditure was on radiolocation. The magnitude of the overall research, development and design effort may be illustrated by figures from the United States of America where 10,000 man-years and $200,000,000 were expended on this less expensive portion of the total radar effort in five war years.

The Scientific Background

Radiolocation grew, as all good engineering grows, from the application to a user need of a system embodying contributions from a wide background field of knowledge and technique. The background of radiolocation is the whole background of that radio science which began effectively with Michael Faraday and developed through Clark Maxwell and through classical experimental demonstrations by Hertz. It has, as outstanding features, the work of Hertz on the reflection of radio waves, of Appleton on radio range-finding, of the British Post Office and the Bell Laboratories in observations of interference with communication channels by wave trains reflected from aircraft, and it includes in particular the powerful radio-pulse technique which was introduced by Breit and Tuve (on a previous suggestion by Swann and Frayne) very soon after, and independently of, the world's first radio range-finding operation carried out by Appleton and Barnett. The background included also the work which was carried out within the programme of the Radio Research Board of the Department of Scientific and Industrial Research, on the special measuring techniques which were used in ionospheric research, in investigating the fundamental properties and principles of direction-finders, on the angle of incidence of down-coming waves, on the propagation of radio waves in general and on the nature and origin of atmospherics. This basic research work under the auspices of the Radio Research Board had led to a considerable variety of applications of the cathode ray tube oscillograph in radio research. I would refer in particular to the approximately linear time bases used in the work of Appleton, Herd and Watson-Watt on atmospherics and the cathode ray 'stroboscope' which was developed by Bainbridge Bell.

What was necessary for the emergence of a new system from this very fertile background was a new and urgent realisation of a specific and pressing need, combined with an imaginative synthesis of elements taken from a store of long available knowledge and technique. Thus I would admit, as founder members of the Radiolocation Club, A. P. Rowe, H. E. Wimperis, Henry Tizard, Philip Cunliffe-Lister, A. V. Hill, P. M. S. Blackett, Robert Watson-Watt, A. F. Wilkins, Adolf Hitler and Hermann Goering. I would deny admission to Heinrich Hertz, E. V. Appleton, even to Breit and Tuve, and even to T. L. Eckersley. Each of these holds an exalted and most honourable place in a wider and more fundamental circle. They belong to the highest class of the 'makers possible'; the criterion for membership of my hypothetical Radiolocation Club is that of the 'makers to happen'.

It would be tedious to repeat the story of how, from inquiry on the practicability of a 'death ray', there emerged at once the posing and the quantitative solution of the problem of location by radio of distant and non-cooperative aircraft, and how one month of concentrated effort by a very small but very select team established the immense potential power of the new art.

The basic arithmetic of radiolocation was simple; the basic engineering was clearly practicable; the economics were formidable. We could foresee the early availability

of one hundred kilowatt pulses on fifty metres, perhaps on ten metres; we would count on developing receivers sensitive to a micro-micro-watt and on noise levels below this value.

In the overall result it was clear that ranges over 150 miles for the location of high-flying aircraft were attainable, that aircraft at 3,500 feet should be located from some 45 miles range without prohibitive engineering difficulties, but that additional provision against low-flying aircraft would be required.

We turn to the second basic medium, the engineering industry of this country in general and the radio industry in particular. The civil engineering and building services of the home coastal chain alone were to absorb nearly £10,000,000 of capital expenditure in the period 1935-45.

Before these major operations could be undertaken the little group of 'Islanders'—the first party who were to undertake radiolocation research at Orfordness comprised A. F. Wilkins, E. G. Bowen, L. H. Bainbridge Bell, assisted by J. E. Airey and G. H. Willis—and their steadily but not rapidly growing group of colleagues had found the possibilities and limitations of their project determined by the available radio components, from valve to resistor. The basic radio tools were available, but they needed reshaping and sharpening, and the skill and experience of the British radio industry met triumphantly the crescendo of demands which ended only with the final collapse of the enemy. The Royal Air Force alone raised its demands for radio equipment from £176,000 in 1935 to nearly £12,000,000 in 1940, and the pre-war radio industry of £20,000,000 grew into a wartime industry of £120,000,000 per annum. The Government laboratories, the university laboratories and the industrial development laboratories co-operated and complemented each other's effort in the breaking down of the barriers set by component limitations.

The third basic medium, the interplay of operational and technical experience and opinion, was our real secret weapon. In its use we far outran the enemy, started far ahead of our American ally, converted him to our faith and works in this direction, and were eventually (in my personal view) outdistanced by him in the last year of war even in the European theatre.

The Milestones of Evolution

A round dozen of the technical milestones in the evolution of radar may be cited briefly and given very rough dates to set the time scale of that evolution.

The first 'prospectus' of radiolocation* had proposed location by measurements of distance from two or more stations, to be supplemented by cathode-ray direction-finding and height-finding. The lack of direction-finding aerials of adequate sensitivity has already been mentioned, and it was a source of grave concern in the first few months of planning that the range-cutting method was liable to give quite false indications, through the association of a range from station X on aircraft A with one from station Y on aircraft B, to give an apparent fix on a non-existent aircraft C. It was, therefore, a major landmark in progress when, in the autumn of 1935, it was established that range, bearing and angle of elevation could be measured and unambiguously associated, for each of a large number of aircraft, by a single combined transmitting and receiving station. *The monostatic radiolocation station* measuring range to 1 kilometre and bearing to $1\frac{1}{2}°$ at 60 kilometres was a product of 1935. Measurement of angle of elevation to $\frac{1}{4}°$ was added early in 1937.

* A memorandum on the detection and location of aircraft by radio methods submitted to the Committee for the Scientific and Air Defence on February 27, 1935.

The same prospectus had offered *a radar identification system*. The preliminary experimental work by Wilkins and Carter showed that this passive IFF system had an insufficient gain, over the untuned response from the larger reflecting area of the aircraft, to be operationally reliable. Watson-Watt then proposed the first radar responder system, a receiver in the aircraft automatically operating a pulse transmitter which emitted amplified and coded pulses on the same or a displaced frequency. This foundation scheme for secondary radar dates from mid-1936, and it was made practical and successful by the work of F. C. Williams and his team.

Rotating Beams

The inability to swing radar search beams of sufficient intensity and of small angular spread had been regretfully accepted by the pioneers of 1935 as imposed by the poor overall power and receiver gain attainable on the wavelengths which might permit mechanical rotation. From the 'prospectus', which proposed a start at 50 metres but development 'as soon as possible' on the wavelengths under 10 metres, through all the discussions of 1935 to 1940 there was insistence on the need for working on shorter and shorter wavelengths to supplement, but not to supplant, the longer. But the over-riding demands of secrecy limited the size of the team and the facilities that could be devoted to radiolocation in the 1935-39 period. This team never turned aside from their cult of the third best—"the best never comes, the second best comes too late." And so the Home Chain opened on wavelengths of 10 to 12 metres, and despite A. B. Woods's work at Orfordness on centimetric wavelengths in 1935, and Larnder's pressure for centimetric trials in 1937, it was not till the raid on the physics schools in 1939 that the conditions for the birth of centimetric radiolocation were established.

Meanwhile, however, Butement, adopting techniques for generating and receiving $1\frac{1}{2}$-metre waves which had been evolved by Bowen's team on airborne radiolocation, set up the *first rotating beam radiolocation station* at Bawdsey in 1939, primarily for ship location as an aid to coast artillery, but immediately seized on as the solution to the problem—also recognised in 1935—of the low-flying aircraft. Butement advanced the precision of radio direction-finding by nearly two orders of magnitude in a single step, achieved early in 1939. His equipment gave a probable error in measuring bearing on a fixed object on the land, on a still day, of 0·97 minutes of arc. The production models gave accuracies, on fading ship targets, of 10 minutes of arc.

A corresponding advance in *precision of range-finding*, was achieved by Pollard in work, culminating in 1938, on the radiolocation equipment for the laying of heavy anti-aircraft artillery. On the first experimental trial of the prototype GL Mark I the errors at all ranges were less than 25 yards. The foundation for precision ranging in fire control was thus firmly laid in 1938.

To turn temporarily from ground equipment, Bowen's ingenuity and persistence, well supported by his team of workers on airborne radiolocation sets, gave us the first fulfilment of the promises made in February 1936 by Watson-Watt, speaking for his small research team. In September 1937 an Anson fitted with 'RDF II' (which later became AI and ASV), on a wavelength of one metre, picked up H.M.S. *Rodney* and the aircraft-carrier *Courageous* at ranges over 4 miles, and also followed aircraft which were seen (by radiolocation) taking off from *Courageous*. Very soon, range accuracies to 100 yards, and total ranges of 9 miles, were attained. Thus *airborne radiolocation equipment* for the detection, location and shadowing of surface vessels was among the established products of late 1937. At that date the maximum range of location of an aircraft by AI was 5,000 yards.

The first potentially operational AI equipment was installed in May 1939, but its performance was not sufficient to obviate the use of a further type of ground installation, developed under the short name of GCI (ground control of interception). This was basically the Butement CD/CHL set, but it embodied height-finding provision on the same principles as the CH set, and it had also one of the most revolutionary devices in the evolution of radiolocation, the *plan position indicator*, PPI. This was another of the dreams of 1935, sternly put aside in favour of a first rudimentary cover by CH, but clearly recorded in a note by E. G. Bowen. The attainment of a satisfactory PPI awaited the reinforcements of 1939-40, and was of the greatest immediate importance in the climax of the battle with the night bomber in late 1940 and early 1941. Its value and power as an operational tool were dependent on sharp directivity in the receiving array; it was very valuable on $1\frac{1}{2}$ metres, more valuable on 50 cms., and advanced by what in this case can only be rather loosely described as another order of magnitude with the introduction of 10 and 3 cm. working.

The CHL/GCI devices embodied another technical advance which became almost universal. This was the use of a *common aerial system* for both transmission and reception. Ranzi in Italy introduced this device in ionospheric sounding by pulses; it was one of the ancillary advantages of the pulse system that it invited and facilitated this artifice. The much higher peak powers and voltages in the radiolocation transmitter called for a high degree of protection of the receiving circuits, and this was attained by a combination of very ingenious devices. The carry-over of these 1940-42 techniques into centimetric working was vital to the success of airborne centimetric radiolocation.

During the period of AI/GCI development in which several of these milestones appeared, the consolidation of the CH system was continuing. One technical milestone only from that route will be selected for notice here. The 'prospectus' to which I have already irreverently referred remarked on a known advantage in pulse working, that one can "obtain, by superposition of the successive images on a synchronised time base, a very easily visible, sustained image permitting close measurement and even showing the advance of the aircraft". This very easy visibility was, of course, due to a cumulative preferential display of the desired deflection against a non-cumulative background of random noise due to other non-synchronised signals, atmospherics, and circuit and valve noise. Among the non-synchronised signals to be expected were deliberate jamming signals from the enemy. I therefore proposed in or before 1938 that we should enhance this normal cumulative preference by the use of cathode ray systems giving discrimination, preferably by colour, between sustained and non-sustained deflections. The meeting of this demand in an elegant proposal by Professor T. R. Merton, and in its working out by the tube development laboratories in the industry, was a milestone not merely for CH but for radiolocation generally, and belongs to the 1938-39 period.

A radar system for hyperbolic navigation was proposed by Dippy in 1938; its merits and promise were recognised by his Bawdsey colleagues, but the fierce concentration on a first defensive cover forced the reluctant but calculated decision to leave it undeveloped meanwhile. Its later and full flowering as the most extensively fitted of all radar systems, Gee, makes the proposal of 1938 one of the selected milestones of radiolocation.

The last of these somewhat arbitrarily selected early landmarks is more than an ordinary milestone. The centimetric revolution came by the same process that brought the radar revolution; the scientific worker went out from his laboratory to meet the military user on his own ground, measured for himself the magnitude of the need, and went back to the laboratory to meet the need from his self-replenishing stock-room of basic scientific knowledge, technique and initiative. If I dismiss this milestone with the brief note that the fulfilment of aspirations of 1935 came through the attachment of many of the best physicists in the country to the coastal chain of early 1939, it is only because the milestone is too big to be brought inside the Pantheon.

And if I leave synthetic trainers and test-gear out of the list of milestones it is because they are not the milestones but the hard core of the highway of evolution in radiolocation.

And so back to the radio industry, which has the knowledge that it was the real Shadow Industry, that its radar and non-radar products multiplied by a large factor the war-waging value of every arm with which they were associated, and that in effect it turned out invisible fighters, bombers, warships and batteries far outnumbering the visible output of the aircraft, ship-construction and ordnance factories.

It may be said with complete confidence that every radar-equipped combat aircraft, ship, gun and light was, after every offsetting factor has been allowed for, much more than doubled in its effectiveness by its radar aids. The radio industry, from the university researcher to the canteen worker and the works labourer, has the deep satisfaction of having permitted us to make the task of our incomparable fighting crews many times easier and more productive, to save from premature loss that irreplaceable combat experience which was the final and indispensable stage of war training, and to save the lives of a great army of our most highly skilled fighting men.

(Abbreviations stand for: AI, Air Interception; ASV, Air to Surface Vessel; CH, Chain Home; CHL, Chain Home, Low; IFF, Identification, Friend or Foe.)

Since 1939 it has been easy to forget the truism that E. C. Large expressed in the words "in war as in peace, man is still engaged in a life and death struggle, not only with wind and weather, but with the multitudinous forces of the insects, the fungi, the bacteria and the viruses, for survival and increase." This article deals with certain aspects of that struggle, and some of the attempts that man has made to alter the natural balance between soil organisms in his favour.

The Control of Soil Organisms

D. P. HOPKINS, B.Sc., A.R.I.C.

Bacteria, fungi, protozoa, insects, slugs, worms—the population of the soil is as much a part of the soil as are London's eight millions an inseparable part of London. To concentrate attention upon the more measurable chemical aspects of fertility and plant-growth is to deal only with one side of a complex and dynamic equilibrium. The farmer pays about £10 a ton for sulphate of ammonia, but it would not be worth tenpence were it not for the billions of nitrifying bacteria who change its ammonia-nitrogen into the nitrate-form the plant can assimilate. Scientists may specialise and set up departments but the soil itself blends physics and chemistry and biology and produces many diverse patterns. Necessary though it is for specialists to isolate each factor in the various equilibria of soil dynamics, it must not be forgotten that a certain amount of de-specialisation is also necessary to interpret the 'balance' and correlation of the various factors.

Under what are usually called 'natural' conditions—e.g. forests, wild grassland, marshes—the various soil organisms have undoubtedly reached a balance in their many competitions that is almost static. When man intervenes with axe or plough or drainage scheme, this steady, hardly changing balance is upset. With continued control of the kind of vegetation that the soil supports, the balance is even more disturbed, and a point can be reached when this balance begins to operate against man's purpose, which is, of course, to feed himself adequately with a minimum effort. At such a point it obviously becomes necessary for man to control not only the crops to be raised from the soil, but also the organisms involved in the life of the soil. He must encourage those that are favourable and discourage those that operate against him.

One direct aspect of this subject is the combined attempt of chemist and biologist to find materials which can be applied to soils to kill or reduce the unfriendly insect organisms. After some seasons of man's cropping, the soil undoubtedly tends to contain increasing numbers of those insects that also enjoy man's kinds of food.

Whatever the 'chemical' balance of such a soil may be, the 'organism' balance has become unfavourable. But it is no mere matter of finding a poison for the enemy classes, for many vital friendly organisms are co-inhabiting the same soil. Mice can be driven from a house by setting fire to it, but it is more selective to set traps or keep a cat. A brief account of the search for a reliable soil insecticide will, at any rate, illustrate the complexities of this type of problem.

What kinds of troubles can be ascribed to the adverse activities of soil insects? Everybody knows of the wireworm evil, and this is not peculiar to British crops or soil. Wireworms abound in temperate and tropical zones alike, and a conservative estimate of world food losses due to this pest would be formidable. A more specific soil organism drew attention to itself in the late 1860's when the French wine industry all but succumbed to a sudden disease that withered the vines, stopped the ripening of the grapes, and left the vineyards filled with dead or dying plants. This disease—phylloxera—was found to be caused by the direct attack upon vine-roots by female 'plant lice' who burrowed into the roots and diverted the nutritional stream into their own stomachs. It was an underground movement that cut off the moisture and food supplies of the plants. Various kinds of beetle-grubs are world-wide pests to man's crops, and in the U.S.A. the so-called Japanese beetle-grub often devastates grassland by its root actions. In tropical zones what are called white grubs inflict the most severe damage upon sugar-cane.

These are by no means all of the larger soil organisms that our crops have to fight against. But a short list is enough to demonstrate a very vital point. They are different in size and habits; they can move about in most cases with plenty of vertical and lateral freedom. How much easier to spray the foliage of plants or trees to kill static eggs, or to set up in advance adverse conditions on leaf surfaces for some seasonal and likely invasion by spores or flying insects! It has been difficult enough to develop foliage sprays that are fairly universal in anti-pest action. How much more difficult to devise soil insecticides that will eliminate a number of pests without also removing vital collaborators whose activities are carried out in the same place!

Indeed, the complex difficulties of fighting soil pests with selective poisons has led to other solutions in specific cases. With the vine phylloxera, initial efforts with carbon bisulphide met with a little success, but the French wine industry survived because a more effective method turned up. It was found that American vines were immune to this soil aphid. But American grapes would have ruined French wines almost as certainly as the phylloxera unless drastic adjustments of the human palate could be persuaded. So the botanist intervened and grafted French vines on to American root-stock. The rapid introduction of grafts on American root-stocks starved out the insects, for without the roots on which they could feed their numbers declined. The speed with which this problem was tackled in a collaboration between French and American science was a triumph for nineteenth-century horticulture.

Today the wireworm trouble is largely tackled by the method of counting before cropping. During the war, with so much old grassland turned into arable land, a practical approach to this problem became essential. It

is of little national benefit to spend labour, seed, and fertilisers upon soil that is so heavily endowed with wireworm that only poor cropping can be achieved. So wireworm surveys have been conducted from various regional centres; teams of workers have travelled about taking samples, counting the wireworms, and estimating the numbers per acre. If not more than 300,000 per acre, then the farmer can go ahead and expect little trouble; 300,000 to 600,000, not so good but fairly safe for cereals; over 600,000, very risky except perhaps for cereals on heavy land; over 1,000,000 per acre, decidedly dangerous for all arable crops.* But although this method of approach has saved a good deal of wasted effort, it is clearly negative rather than positive, acceptance rather than control.

Now these methods are specific, practical methods developed by hard experience to deal with individual pest-troubles. The need for a soil insecticide with a fairly wide range, one that will destroy a good many pests and control most of the rest, is a world need.

Soil Insecticides

What kind of substance is likely to have any chance of success as a soil insecticide? It must be able to spread through the soil and, in general, such movement can be expected only from a vapour. As it would seem impracticable to force gases into the soil, the insecticide must be the next best thing—a solid or liquid of high volatility, the vapours from which will spread to effective distances around points of application. And this is not as simple to arrange as it sounds. The diffusion of the vapour will depend upon the porosity of the soil, and the type of soil and its wetness will clearly dictate the porosity. Also, the soil temperature will be a major factor in vaporisation; a substance that gives good results in California might prove quite ineffective in East Anglia. Though it should not be concluded from this that *rapid* vaporisation is an asset. With rapid vaporisation, the top few inches of the soil may never hold a sufficient concentration for pest-control; since the effectiveness of an insecticide must depend upon (*a*) its concentration and (*b*) the period for which it can be maintained. The soil insecticide must have the concentrated attack of a blitz over a reasonably wide and deep zone and it must persist for a reasonably long period.

Then it must be a substance that can be easily applied in large-scale practice. A chemical that can be handled only in small-scale laboratory tests is hardly promising. Though liquids are likely to fulfil the vaporising function more efficiently, solids are very much easier to apply. Solids can be broadcast on the surface, then mixed into the soil by ploughing or discing. It is probable that the ideal soil insecticide (if one ever does emerge) will be solid. Nevertheless, the best-known and oldest soil insecticide is a liquid—carbon bisulphide. But, except for the small, intensive scale of glass-house cultivation, the liquid soil insecticide seems gravely handicapped. Indeed, so much more practicable is it to apply solids that recent research efforts have been in the tentative direction of preparing liquid insecticides in the form of impregnated dusts or jellies.

* See *Wireworms and Food Production*, Bulletin 128 of the Ministry of Agriculture and Fisheries. (Stationery Office, 1944.)

Wireworm *versus* wheat. The strip on the right illustrates the damage done to the untreated crop; the land on the left was treated with gammexane. (*Photograph by courtesy of Plant Protection Ltd.*)

Then comes what is perhaps the most important consideration of all—*what other lethal properties will the insecticide possess? How much will it upset the favourable as well as the unfavourable organisms?* And, even more obviously, how much will it damage the plants? So far most substances with insecticidal properties have had to be applied at concentrations that are also harmful to growing plants, and this means that the soil cannot be cropped at the same time. However, this is not difficult to overcome if the plant-toxic effect does not persist for more than a week or two. But it is not possible to evacuate the soil's 'good' organisms for the period of insecticidal treatment—friends cannot be protected from the blitz upon foes when all live in the same target area. It might be said here, then, that the insecticide must be selective in action; but, nevertheless, we want an insecticide that will in one application deal with several soil pests, and it is hardly to be expected that a substance which is toxic to a wide range of troublesome organisms will yet be harmless to helpful organisms.

The effect of a general soil insecticide upon fertility processes is a matter of some dispute. The contribution of the organisms to fertility and plant-growth must be fully realised. Bacteria like *Azotobacter* draw in the inert air-nitrogen and make it eventually available as active

plant-food; nitrifying bacteria turn complex organic nitrogen forms into nitrites and then into the nitrates that plants can assimilate. There are also the parasitic bacteria of the legumes that fix air-nitrogen.

There are the earth-worms. It is increasingly considered today that in many of these favourable soil processes the initial stages are mainly carried out by soil fungi, the work of the bacteria beginning only when fungi have paved the way. Can this favourable total of types of change be saved from a substance that is toxic enough to kill pests? Will not the damage to fertility equilibria be greater than the benefits of pest control? This, of course, is a similar dilemma to that of the effect of a foliage poison-spray upon such beneficial insects as bees or ladybirds, and many practical growers are inclined to adopt an instinctive, reactionary attitude towards the insecticide.

For what personal comment is worth, I would suggest that no general discussion of this problem is of value. Each case of application must be considered upon its factual merits. If a soil is of high fertility, and if the pest-damage to crops is not excessive, then it is probably not worth risking temporary interference with fertility equilibria to reduce the pest nuisance. At the other extreme, however, a soil may be so heavily pest-ridden that its intrinsic fertility is of little value since the crops raised feed the pests rather than man; then it would clearly be only common sense to run risks with the fertility in order to reduce the pests. In short, it depends upon the degree of damage caused by unfavourable organisms. Against this some will argue that where there is good fertility there will not be an excess of pests since fertility is an expression of the 'balance of Nature'—but we cannot so readily accept this optimistic view that Nature and Man work towards the same ends.

An interesting example of the effect of a chemical upon soil bacteria has been observed by Dr. Tattersfield. In using naphthalene as a soil fumigant, he noted that doses on previously treated soil disappeared much more quickly than similar applications to soils not previously treated. He found this was due to the presence of a species of bacteria that could feed upon naphthalene. The first dose encouraged the multiplication of this species, so that later doses found increasing numbers waiting to devour them.

So much, then, for a general look at the kind of substance a good soil insecticide must be. In a recent survey of the current research position, Dr. H. C. Gough summarised the qualities of the ideal soil insecticide as follows:

(1) Toxic to soil insects.
(2) If also toxic to plant-life at the same application rate, then this effect must be non-persistent.
(3) The equilibrium of soil micro-organisms must not be permanently upset, though temporary derangement may not be dangerous.
(4) Easily dispersable through the soil, i.e. a solid or liquid that vaporises readily.
(5) Physically capable of easy handling, should store well, and not be inflammable.
(6) Should not disturb plant-food availability in soil, nor react detrimentally with manures or fertilisers.
(7) Cost of substance and application must be within the economics of the crop or crops to be grown.

It is hardly likely that any substance will actually meet all these criteria, and a satisfactory insecticide will be one that satisfies some of them so well that it can be forgiven shortcomings in regard to the others. The practical grower will not expect perfection; if his soil is badly and increasingly pest-ridden, he will put up with lesser evils to overcome the main one.

What kinds of substances have so far been investigated and tried? First place is perhaps still held by the oldest—carbon bisulphide. It is inflammable and it has the liquid's problem of application, but it has, nevertheless, been more used than any other. It may be that the amount of use in practice is only greatest because it has been known the longest. Naphthalene is also much used, often under other names since it is the 'secret weapon' enclosed in many commercial soil fumigants and cleansers. It has the advantages of a solid in ease of application, but it is uncertain in effectiveness. To be efficiently toxic it must be used at high rates, and the usual recommended applications are apt to be below this rate so that failures are frequently recorded. When successful at low rates, it is considered that it has repelled pests temporarily rather than destroyed them—though this repulsion can fairly be regarded as a form of control. Various cyanides have been successful, but their exceedingly poisonous qualities make them dangerous in practical use. Even were the cyanides perfect soil insecticides, it would be wisest not to recommend their general use for greater evils might be introduced. Chlorpicrin has been promising but it is difficult to handle and the toxic effect upon plants is too persistent. In our climate paradichlorbenzene has been unable to win the favour it has found in warmer zones for it would seem to need a rather higher soil temperature than ours for effectiveness.

In his recent survey Dr. Gough gave an intriguing summary of results recorded for various substances. Analysing the reliable experiments for each insecticide, he listed the number of successes actually achieved. Obviously some substances have been given more attention than others by research; nevertheless, Dr. Gough's rough guide helps towards seeing the wood for the trees, and it shows incidentally that some of the simpler substances into which amateur gardeners put a great deal of faith are decidedly 'hit-and-miss'. The table gives some of Dr. Gough's figures.

The wide variety of these names suggests that science has tended to 'try out' possible substances more or less at random. This is true enough, and in a search so clearly in its early stages this approach is defensible. Dr. Gough

	Total No. of Tests	*No. of Successes*
Carbon bisulphide ..	68	55
Same, as emulsion ..	46	28
Naphthalene	53	27
Calcium carbide ..	8	2
Lime	11	3
Nicotine	24	11
Pyrethrum	11	4
Sulphur	7	1
Various cyanides ..	82	51
Chlorpicrin	19	16
Coal tar oils	60	37
Kerosene	25	14

has stressed that this must be the first phase of attack. When, eventually, a large number of chemicals have been tested and their relative degrees of reliability assessed, then the second phase can begin—a phase in which the common factor of success is looked for. Such a common factor—particularly now that more complex organic substances are being examined—may well give a line towards some other substance that has not been examined at all, or even some new homologous substance that can be synthesised. Or it might be feasible to prepare combinations of substances, mixtures in which individual strengths and weaknesses are balanced. Meanwhile, research is, today, still in the opening phase, the more or less haphazard collection of observations and data.

Imperial Chemical Industries announced last year the discovery of a new general insecticide—gammexane or 666, the gamma isomer of hexachlorocyclohexane, or benzene hexachloride. This new weapon in the battle against pests was developed in the search for a substitute for derris. While many foliage and household pests can be effectively destroyed with gammexane, it is too early yet to decide whether it will achieve similar successes in the soil. Dr. Slade—in the 1945 Hurter Memorial Lecture to the Society of Chemical Industry in which many facts about this wartime discovery were released—indicated that preliminary trials had shown that gammexane might be an effective destroyer of wireworm. Certainly gammexane would seem to have some of the basic qualities of a good soil insecticide. It is stable over a wide range of conditions, it does not seem to be harmful to people or animals, it kills apparently by jointly acting as a stomach poison, a fumigant, or a contact poison. Although not readily volatilised, experiments with the grain weevil have shown that a lethal fumigant action can be brought about at normal room temperatures. This would indicate that, in the soil, penetration can be achieved without the risk of rapid loss of concentration—so the hope that gammexane may become a really useful weapon against the mobile wireworm gains some justification from an analysis of its established properties.

It is probably too early to attach great significance to the economic factor. In all exploratory work, the initial balance-sheets tend to look formidable, and costs of materials and handling are apt to decrease once the pioneering stages have been passed. Costs of chemicals partly depend upon the size of the demand, and costs of application will often fall if an operation is widely enough performed to warrant the production of practical machinery. Nevertheless, it seems safe to predict that soil treatment with insecticides is unlikely to commend itself to normal agricultural practice where the return per acre is relatively low; at any rate, only the cheaper solid types that are better regarded as repellents and fumigants would seem to fit the agricultural balance-sheet. The farmer will probably have to rely upon other methods of dealing with unfavourable soil insects. But, where the crop-value per soil unit is high, as in the richer kinds of market gardening and in glass-house work, the soil insecticide may well give a good return for its costs. And it is often in just this kind of soil, richly organic and frequently cultivated under artificial conditions, that attack by insect pests is worst.

Soil Sterilisation

Any discussion of soil organism control would be incomplete without including the practice of soil sterilisation. Soil sterilisation is rather a misleading term. At any rate it must not be taken literally. It is a practice applied to the small bulks of soil that are intensively cultivated in glass-house work, and it does not, of course, imply complete sterilisation. A partial degree of sterilisation is aimed at with the idea that more harm will be done to the 'bad' organisms than the 'good'.

After two or three seasons of intensive crop-production glass-house growers often find that the soil becomes toxic. Despite an adequate supply of plant-foods, crop production and plant health falls off. Also, in pot-raising plants from seeds, erratic results occur which can only be due to initially toxic soil-conditions. Partial sterilisation of such soils is an essential operation in efficient glass-house or plant-raising cultivation.

The heat treatment has been given in various ways—direct 'baking' with careful temperature control, steaming either at high or low pressure, and more recently by using the soil as a resistance to the passage of electricity. Early work at Rothamsted found that most weeds, weed seeds, and soil organisms—including the nitrifying bacteria and protozoa—were destroyed at a temperature of 60° C. But the ammonifying bacteria survived even 100° C. This specific survival has a considerable effect upon the immediate fertility after sterilisation. The protozoa—the enemies of the ammonifying bacteria—have gone. There is, therefore, a doubly favourable condition for a rapid multiplication of the ammonifying organisms. Added to this is the fact that heat treatment tends to change some of the more complex forms of organic nitrogen of the soil into forms that are nearer to ammonia in the sequence of conversions. The soil solution, therefore, becomes excessively charged with soluble ammonia-forms, which is not helpful since the nitrifying bacteria have been severely reduced. The remedy adopted has been a post-sterilisation addition of superphosphate, which takes up some of the ammonia excess. It has also been found that the 'check' in fertility after sterilisation is greater when the soil is high in lime content or organic matter content. Both of these observations fit the theory that the check is due to an excess of ammonia, for lime would, of course, throw ammonia more readily into solution and an initially high organic content would provide more forms of nitrogen to be worked upon by the stimulated ammonifying bacteria. When the heating is prolonged, the simplification of the complex nitrogen forms is apparently greater, so this also will increase the check after sterilisation. The degree of sterilisation recommended by Lawrence and Newell, who investigated these problems at the John Innes Horticultural Institution, is a soil treatment at about 82° C. for a ten-minute period.

Chemical methods of partial sterilisation have the advantage of being less cumbersome than heat treatment. A 2% solution of commercial formaldehyde is the most widely used. Cresylic acid and tar-oil products are also used. Formaldehyde is efficient as a soil fungicide, but fairly ineffective as an insecticide; but many of the toxic conditions that develop in glass-house soil after over-use are due to fungoid organisms, especially in

tomato cultivation, so that in practice formaldehyde tends to 'get by' without unduly revealing this limitation. The other chemicals mentioned above are efficient in insect control but ineffective as fungicides. After chemical sterilisation there is a delay of a few weeks before the soil can be used for plant-growth. The practical considerations already discussed in regard to insecticide applications also apply, and the glass-house grower usually covers the treated soil with sacking, etc., to prevent dilution by surface evaporation. Experience has indicated that the relative efficiency of the methods of sterilisation are; heat, 100; formaldehyde, 90; cresylic acid, 80. But, while the heat treatment need be given only once each three years or so, the chemical methods should be carried out each year; so that these indices of comparison are somewhat flattering to the chemical sterilisers.

What happens after sterilisation? Does the original balance of organisms return to normal after an interval, or is it permanently deranged? Bacterial count immediately after sterilisation has shown, as would be expected, a big drop in population. But, after a short interval, it is found that the population has risen to a much higher count than the pre-sterilisation figure. This, however, is a quantitative view. It is highly probable that the balance of various favourable species is changed for many weeks. But the need for heat sterilisation every third or fourth year indicates that there is a gradual return to, first, some kind of 'normal' balance, and then to the less favourable balance built up by intensive mono-culture, the balance that again needs sterilisation. Indeed, the practical success of soil sterilisation, recorded by many commercial growers and nurserymen, strongly suggests this is by far the best step forward science has yet made in the general attempt to control soil organisms.

Another ramification of organism control possibly will emerge from the study of plant symbiosis. An increasing amount of attention is being given to the mutual assistance of certain plants when grown in proximity, and to opposite cases where one kind of plant would seem to inhibit the growth of another kind. It is believed that soluble substances—antibiotics—are discharged from plant-roots and that this phenomenon is of widespread occurrence and significance where higher or lower organisms are competing in the same zone. Sir E. J. Salisbury recently pointed out that soluble antibiotics from root systems might have considerable practical application; presumably he was speculating upon the isolation of these substances and perhaps also upon their laboratory synthesis, thus enabling them to be added to a soil externally.

Finally, it should be said that this article has not attempted to be comprehensive. Probably as many developments or possible developments have been left out as have been mentioned. In the words of Charles Thom and N. R. Smith in a paper in the U.S.A. *Year Book of Agriculture*, 1938, "Constructive experimentation is required to determine just what microbial activities are needed for particular crops in each soil group and how those activities may be maintained at desirable levels. Correspondingly, the existence of unfavourable microbial activities in many soils is already known. Much experimentation will be necessary to establish means of elimination, of control, or of replacement of undesirable by desirable species."

Science in the Air

Last month the Ministry of Aircraft Production organised two interesting exhibitions at which various technical developments made during the war were displayed. At the Earl's Court exhibition manufacturers and research workers had the opportunity of seeing some of the electronic developments made in Germany during the war. A wide range of electronic equipment collected by our technical missions to Germany was on show.

From the radar exhibits one gained the impression that the Germans had always lagged behind Britain in this field; here was the evidence for wartime propaganda about technical superiority which was not always accepted at face value. One section was devoted to guided missiles. At one time the Germans were developing nearly 50 types, but only one or two came into operational use. Proximity fuses under development included acoustic, electrostatic and photo-electric fuses. The Hs 293 glider bomb was on show; this bomb was guided by a parent plane, by which it was trailed at the end of a great length of steel wire.

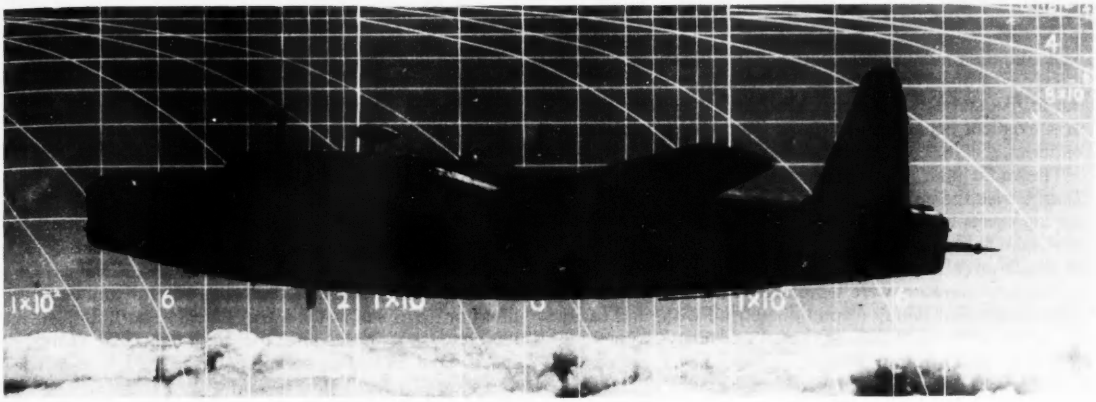
The Germans concentrated a great deal of effort into infra-red equipment. On exhibition was a sniper's rifle for use at night, which depended upon an infra-red spotlight which 'illuminated' objects in its beam, and these were then visible when viewed through the attached telescope. The telescope's optical system focuses the infra-red on to what is called an 'image converter tube', which operates in a similar way to a combined television camera and receiver. A tank equipped with a similar arrangement was also shown.

There were D.K. cells from the Heidelberg laboratory of Professor Wesch, one of Lenard's followers. These cells contain crystals of cadmium telluride and cadmium selenide (in solid solution) that are sensitive to infra-red. When infra-red falls on these cells the change of dielectric constant can be registered electrically.

'Kiel', an airborne equipment for detecting an aeroplane's exhaust, was also on view, as was 'Madrid', a heat-homing missile. Both these depended on lead sulphide cells sensitive to infra-red.

The other exhibition was at the Royal Aircraft Establishment where techniques devised in the Metallurgy Division were to be seen. There was a demonstration of the 'magnetic ink' used to detect cracks in steel parts; a mixture of iron oxide in alcohol, the 'ink' is poured over the part under test, which is then magnetised and the particles of the oxide stick to the cracks which are thus made visible. In collaboration with instrument firms, ultrasonic instruments for detecting internal cracks have been developed.

The R.A.E. metallurgists have perfected methods of glueing together metallic sheets with plastic cements, the bond being strong and durable. One section of the laboratory has been concerned with corrosion problems, and has overcome many difficulties involved in the protection of aluminium alloys and special steels. In the Tropics paints lose their protective usefulness, but surface treatment with chemicals, particularly chromates, considerably lengthens the service life of metallic structures. The work of the Metallurgy Division is not spectacular: most of it requires time and patience. It is impossible not to be impressed by the enthusiasm of this large team of well-trained scientific workers, most of them under 30 and led by a brilliant young man, Dr. Bruce Chalmers. Here, one feels sure, are some of the future leaders of industrial metallurgy.



Atmosphere—May 1941

J. M. WALDRAM, B.Sc., F.Inst.P., F.I.E.S.

Wartime research has taken scientists into some unusual places and on many strange missions. Here is an impressionistic account of the background to one wartime experiment. It was not originally intended for publication, but was written by the author to entertain his colleagues. We thought it worthy of a wider audience

We got down really early to the aerodrome that night. We had always intended to; but on every previous night we had underestimated the time, and had had to start in a rush. Someone pleaded that so early a start was unnecessary but I was adamant. To be unhurried makes a great difference on a complex experiment, and this one was as complex as anything we had ever attempted; the previous night's effort had not been too satisfactory. Photometry at low brightnesses is not easy even in the lab., but we were to do it at night in an aeroplane, with bad vibration and low temperatures, at heights up to 20,000 ft. and encumbered with harness and masks. We should do well to get any readings at all; if they were reliable it would be very good indeed.

We got to the Wellington after a four-mile drive in our hired Irish Austin, in the fading light of a beautiful May evening—Sparke of R.A.E., Crawford of N.P.L., John Morse and myself from Wembley. The crew had not yet started their final check, which was as well; for once they begin it is difficult to get into the aeroplane or to do much when you are in. We had done as much as possible in the afternoon, but it was important to see that things had not been shifted.

There were four separate experiments to do, each of which had its own gear, which in two instances was elaborate and needed precise setting. In the nose was a contraption involving a telephotometer and a pair of field-glasses, which John and Crawford were to work. It was the bane of the whole expedition. On two previous trips the instruments had been pushed out of alinement by the turret door, which had blown open once in the air and had once been opened by an unexpected passenger during the day. Tonight they had been screwed up doubly tight after particularly careful alinement, and the door had been lashed up with rope and another cord tied across to keep off stray aircraftsmen. In the back was a whole collection which I was to work, the star turn of which was a fearful instrument which Collins had made. It was the Nephelometer—its name alone stumped most people, and its optics often enough stumped me. It included the lab. photometer, clamped in position and particularly carefully set and screened; I was never satisfied with its setting and was always checking it. There was some gear of Sparke's, set in the side of the aircraft, which was quite simple and gave no trouble; a portable low-brightness photometer which I was to use, from the astral hatch amidships, on the beam of a searchlight, which the searchlight team was running for us on the ground. I also had to use the photometer through a little window in the floor at the back, read a wet and dry bulb thermometer, and various oddments.

The big Wellington was ideal for the job. There was plenty of room everywhere—for an aeroplane—and it had all the necessary facilities such as intercommunication, oxygen points and cabin heating. It was easy, if necessary, to make holes in the fabric, in a way that is impossible in a 'stressed skin' machine.

As it turned out, John and Crawford were having by far the most difficult job; my observations made up for difficulty by their number and variety. Sparke had the responsible job of recording and directing the work, sitting at the observer's table and receiving our readings by 'phone. We had worked out a very careful cycle of operations, for we could not use our radio to the ground for various reasons, and signals were to be made with the landing-lamps. It sounds easy; but once you get out of step you are sunk without hope.

Not even the radio men had appeared, and we had the plane to ourselves. We checked the telephotometer again. We made sure that the photometer batteries were 'up', that the instrument lights on the plane were adjusted, and that the bomb-aimer's window, through which we were to work, was clean. (It got oily from the front turret which was inclined to leak on this machine.) John and I went

back and did the calibration of the nephelometer, and made the series of observations at ground level after we had reset the instrument for the umpteenth time. We stowed gear in convenient pockets and stowages and checked the main batteries.

The radio man squeezed in and played with his gear. Sparke tried the intercom., tested the oxygen, and arranged his log sheets in his 'office'. The main control rods near me began to move about, and I realised that they had come to check over the aircraft. (Those control rods, to the rudder and elevator, were temptingly close to my position in the back, just handy to pull oneself up with, and easily mistaken in the dark for the hand-rail. It says something for our concentration that they were never pulled by accident.) The orange-red cabin instrument lights went suddenly out, as the starter truck was connected.

"Contact port!" Faintly from outside.

"Contact." The starter whined and sighed, and the port engine started with a splutter and ticked over. Peace and quiet had gone for the whole night now. The whole machine shook and vibrated, and I went round tightening things up. John came back to the bed where the flying-kits were stowed and climbed into his.

"Contact starboard!" The starboard engine started and the noise and vibration increased. I took off my shoes, and climbed into an unmanageable flying-suit with a teddy-bear lining, half a mile of zip-fastener up the front, and a great fur collar. Heavy fur-lined flying-boots. John taps me on the shoulder and points to his—a zip-fastener has stuck. (One can't converse above the engines even when they are only ticking over). I bend stiffly and get it partly right for him. He puts my harness on my back for me, tucks the 'tail' between my legs, and the four catches go home one after the other with a snap. We inspect each other's. We are now about as nimble as deep-sea divers.

One of the engines opens up, steadily building up to a deafening roar; the whole 'plane shudders, the tail bounces about in the slip-stream, and we keep an anxious eye on the gear. I find my helmet and disentangle the 'phone cord and oxygen pipe, put it on and plug in the 'phone. The roar of the engine is muffled. It is a queer sensation when the 'phones come alive; a little world of sound of our own, unaffected by the noise outside. Without the 'phone conversation is impossible even if you put your mouth to the other man's ear and shout.

The port engine shuts down, and after a moment of comparative calm the starboard engine comes up to full throttle while they satisfy themselves about revs, pressures and what not. It comes back to a tick-over.

A microphone is switched on somewhere, and a voice speaks in my ear.

"Hullo, Sparke calling Waldram. Can you hear me O.K.?"

I switch on mine.

"Waldram answering. Yes—O.K. Can you hear me?"

"O.K. Sparke calling Crawford . . ." I switch off and listen to the check-over, for the whole experiment depends upon that intercom. working properly.

The skipper has arrived and is in his seat; the wireless operator with his bundle of log books, prepared once more to be sick in the cause of science. (He was a ground man normally.) The late twilight is still there, the distant hangars are getting hard to see. Through the cabin windows the red obstruction lights and the white flare-path lights on the runway are showing up against a deepening emerald sky. The door slams, and faint shouts are heard outside. The rods move again as the skipper feels the controls. A roar from both engines and a hiss of compressed air from the brakes, and we move away to the end of the runway, the brakes squealing as the machine swings neatly round to face straight up the long wide flare-path. A last look round; he always goes quietly over the cockpit for a full half-minute. A green flash from the A.C.P. glints on the perspex. Then the two engines roar into full and deafening life; I brace myself at the astral-hatch. The floodlight and its crew slide past, and the flare-lights, at increasing speed—and now the bumping has ceased, the last few flares are fifty feet below us and we are airborne.

A spate of morse comes through the headphones.

Behind is the Lough, shining in the last light; below, the grey aerodrome, the irregular pattern of the steady red obstruction lights and the winking red square at the wireless masts; the beacon is flashing steady morse some two miles away. The whole scene tips up as we come round on a banked turn, and the port wing dips into the countryside. There are stars over my head, and there is still a green light in the west, vanishing into purple haze. The Lough is below us now; to the right are the Antrim hills, dark in the east and cloud-capped. Details fade as we climb.

There is plenty of time yet, for we are to start work at our ceiling, and it will take an hour to reach it. I sit on the bed for a rest; one can't talk—I am alone in the back, and the noise drives one's thoughts in upon one's self.

This trip is the culmination of a great effort. Two months ago we came over in a Boston to try to do this job; but the machine had one series of faults after another and we went back disconsolate by train, without ever having been off the ground at night. Then we got our Wellington, after some troublesome delays; the workshops did some fine carpentry, we made some lab. trials with that telephotometer (these involved observing a toy motor car running about on the floor of the lab.) and at last, after some more maddening delays, we got over. All seemed set fair. Then, a few nights ago, as we were taxi-ing to the runway for our second flight, there was a sudden 'crump' and a crash, and we stopped with a jerk. By some mischance we had been rammed by a Beaufighter. Nobody was hurt, but our aircraft was completely unserviceable. The Wellington was towed off the runway with difficulty, and we got the more valuable gear out and on to a tender, and went sadly home. . . .

By some miracle a Battle had been produced for us next day, in which Crawford and I made a gallant but futile attempt at some visual observations. He tried to observe in a shower of exhaust sparks from the back, and I held the telephotometer in my hand, looking through a hole in the floor in a blast of hot air from the radiator.

We had been sent home by an air-raid without one result. Then—another miracle—we had been given a new Wellington, and the fun had really begun. The four of us unaided had stripped a whole lorry-load of gear, carpentry and fittings of all kinds from the damaged machine and screwed them down in the new one. Of course, many things failed to fit, and, worst of all, that wretched telephotometer, which had originally been arranged nicely in the back

where we could move it in comfort, could not go there in the new machine. We had to improvise a place for it in the front. This meant another observer, and Crawford, who had come over solely for some observational trials and was not really concerned, had volunteered to come and work it, in horrible discomfort. We had had to find a helmet and mask for him, and he had no proper flying-suit. Where we should have been without him I cannot imagine.

The labour of that change-over had been terrific. We had blessed the roominess and convenience of the Wellington, but even in that machine there was limited space, and aircraft work generally needs special tools. Anything that is dropped in a Wellington goes into the geodetics and is lost at once.

The job had taken nearly three days; we had made interim flights, and here we were on our second shot, in our fourth aircraft, with some hope of getting results before the moon came up and we should have to stop. It had not set yet; if we failed tonight we should have to stop and go home.

The river showed silvery where it reflected the last light. Two flares, just visible, marked the scene of our operations on the ground. We zigzagged over them, with wide turns, gaining height. At one end of the beat we could see the beacon of another aerodrome, and the coast line of the bay faintly. Meaningless morse signals twittered and jabbered in the telephones. Suddenly the searchlight was on, standing straight up from the ground like a silver pencil, and going up and up above us, looking almost solid. For forty minutes we climbed.

"Sparke calling everybody. Ten thousand; you can put your oxygen on now."

"O.K." in chorus. I took the end of my pipe from my knee pocket and put the plug into its socket, marked by a little row of luminous dots. One doesn't need oxygen at 10,000 ft. but the lack of it brings the visual threshold up quite significantly, so we were cautious.

The stars over my head slewed a little and revolved solemnly in the astral hatch as we made another slow turn. It was getting cold, and I cuddled the hot-air pipe which blew warmly up my legs. Out on the great wing the port engine roared tirelessly; I could just see its dark shape, and occasionally a glint on the airscrew. At last the pilot announced that he was up to ceiling as near as made no matter, at 18,000 ft. It should have been higher, but the engines were running hot. From the front their two exhaust rings glowed red like dim fiery eyes, notwithstanding a 180 m.p.h. wind. Now we started.

I stood up in the astral hatch with the photometer. The searchlight canted slightly, taking the flares with it, and then straightened up and sailed towards us. I was following it with the photometer, and read its brightness as we ran by it, a few hundred feet away, a ghostly column. Some sky readings followed. Sparke asked the pilot to signal to the ground with the landing-light, and I went aft. This sounds simpler than it is. I was standing on a kind of box, with a padded wooden rest encircling my chest and supporting the photometer and control box. The drill was complex and the execution ungainly. Switch on my cap-lamp (a home-made and invaluable gadget) and disentangle the lamp leads, the instrument flex, telephone cord and oxygen pipe. Pull out my 'phone plug. Gather up the instrument and control box. Wriggle out of the ring. Turn off the oxygen, and disconnect the plug. Stagger down the 'plane, partly in the dark. This is really very difficult, for one is dreadfully hampered by the heavy boots, suit and harness; the 'plane rocks about, and one's hands are full of photometer. The cat-walk is not flat but is a series of steps up and down over stowages and controls, ending with a big step down beside the nephelometer, which must not be kicked. The effort, without oxygen, is quite considerable. The oxygen pipe and 'phone leads catch in everything and get trodden on, whereupon one is held by the head. Once I managed to get one of them on one side of a vertical post in the middle of the cabin and the other on the other side, and so tied myself in effectively.

Eventually, and drunkenly, I got back. First the oxygen, and a good breath of it, and the telephones. Another brightness reading through a window in the floor, and report it to Sparke. Then disconnect again, go farther aft, kneel clumsily down, and read with a mirror and the cap-lamp the wet and dry bulb thermometer. Get up, reconnect, report the readings, sit down without tangling the leads, and get to the nephelometer.

Meanwhile, on our signal, the searchlight had gone out, and in its place they had switched on the lights illuminating a 40-ft. white square on the ground. It was now John's and Crawford's turn, and they, if you please, were to measure the brightness of the sheet as we went over it. John and I had practised this in the lab., and it was not easy. A 40-ft. sheet at that height looks about as big as a full stop. The telephotometer has a not very big magnification, and a very small field of view, and into the bargain it inverts the view. It is nearly impossible for one man to aim it and to do the photometry at the same time. So the drill which we used was this. The instrument looked through the bomb-aimer's window with an arrangement of mirrors, controlled by John. He looked through a pair of field-glasses through the same mirrors, and when the target came into view he 'fished' for it, wagging a mirror by hand, and held its image on a luminous mark in his glasses. When he was 'on target', he grabbed Crawford's leg as a signal. If all went well, Crawford, with his head touching John's, could look through the telephotometer and see the image in line, and could measure its brightness. What with the vibration and the swinging of the aircraft it was a feat indeed to get anything. I know; I had tried it myself on earlier runs. So while I was getting on with the nephelometry, the pilot would make three or four runs over the target while they attempted this feat.

I pushed the circuit-breaker, and the lamp ran up, lighting all the tail with 'spill', and I adjusted the volts. The photometer steadied down. The photometry is beastly; low field brightness, with the photometer swaying about so that you lose the field, and just as you are getting a balance there is a bump and the eyepiece hits you smartly in the eye. Still, we had done it before, and readings began to come through. Sparke was logging them at the other end of the machine in his office.

"Seventy—two point two, one over B."

"Two point two."

"Sixty—one point three. Seems low."

"One point three."

"No—one point seven."

"One point seven, one over B."

"Fifty—one point two five."

"What?"

"One point two five—one, two, fife."

"One three five?"

"No, one, TWO—one two—fife."

"O.K." We were interrupted.

"Crawford calling Sparke." (What news?) "Telephotometer reading two point two, one over A." (Well done, you two! I blessed his slow quiet voice.)

"Two point two, one over A. Sparke calling Waldram ——" and so on. Before I had done my lot, John and Crawford had got three readings in, by some miracle of skill and patience. I worked back to 30° and then on from 90° to 150°. Check volts and switch off. Switch on the R.A.E. projector and check the volts. Switch on their photometer and check its volts. Get up and take readings. Switch off again. Another signal to the ground, and the searchlight comes on again, at another current; they have changed carbons in the meantime. I gather up the portable photometer, disconnect, stagger up to the astral hatch again, and connect up the 'phones and oxygen. Again the searchlight beam sails by, and I read its brightness.

That ends one cycle.

"Sparke calling pilot. Please descend to 12,000 feet."

"O.K.—O.K." The nose went down, and the machine shuddered in a dive. Five or six minutes later: "Hullo Sparke—twelve thousand now."

"O.K. Run past the searchlight, please." We get going again, on a new cycle.

All that was repeated, without missing a reading and without getting mixed up, five times. Each cycle took about twenty minutes with about five minutes more for the de cent. The vast night atmosphere became less mysterious as friendly stars rose and set again and the faint red moon set in what may have been the true horizon or may have been haze. I have never been so conscious of the vastness of the night sky as I was, looking out of the astral hatch at the stars, the dim misty darkness, and the silver searchlight and the two tiny flares slowly wheeling below us. The earth seemed puny and remote.

The oxygen gauges went to fifteen, ten, five, and were turned off. We were shooting past the searchlight at couple of thousand feet, just in the ground haze, the fields visible below. The last reading was taken. Three and a half hours' work.

"That's the lot, isn't it?" came Sparke's impassive voice.

"That's the lot. Home, James."

"Sparke calling pilot. Will you take us home now, please?"

"O.K.—O.K."

We made a last cheerful signal to the ground; the searchlight went out, and a torch winked in reply. We could see the paraffin flares flickering on their last pint. The photometer went back into its stowage; I put the big cabin lights on (against all rules) and fished out a large thermos flask. Cup after cup of coffee went forward; very welcome. We took off our masks and munched sandwiches.

Dead ahead of us the aerodrome beacon was flashing, and as we approached, the red pattern of obstruction lights picked out the aerodrome. (Better than last night, when they kept us waiting for an hour before we got in!) We circled round, and I was suddenly blinded by the identification light, a few feet from me at the astral hatch, as the pilot asked permission to land. Round again. Then the flare-path spread out across the aerodrome; a double line of white lights. Round once more. A green flash from the ground.

We are farther out now, over the Lough; the engines are throttled back and the machine seems to have been clutched from behind as the flaps come on.

A fan of light spreads across the aerodrome from the floodlight, picking out silver aircraft and distant hangars. The flare-path is dead in line ahead as we sink towards it. The hedge slides below; the obstruction light on the nearest hangar sails by level with us, just in the same position as last night, to a foot. We are past the floodlight, and a great grotesque shadow races on ahead. Bump—bump, bump. The two great engines come to rest with a clatter of tappets, and there is a sudden, appalling quiet. My ears are singing.

Still some work to do—a last set of readings on the nephelometer and on Sparke's projector for zero height; instruments to be put back in boxes. I turn the knob of my harness and hit it, and the harness falls off. Flying-kits are stowed back on the bunk. A hunt for shoes. Down the ladder stiffly. The maintenance people are picketing the machine for what is left of the night. We drop the operator at his hut, and the pilot at the Mess, and drive wearily back four dark miles to Antrim.

. . . . .

Crawford and I were lying on a grass bank by the Lough in the warm sunlight. We were on the edge of the links, and occasional drives landed with a lazy plop near us on the grass. The sun glinted out of a blue sky on to the sparkling water. I gave another loving look at the set of curves that we had just drawn; a good set, with the points plotting consistently. Even the telephotometer readings, so hard won, made a good show. We shut up the book and put the papers away. Then Crawford led the way through some fields to a little stream, and began to talk about caddis-worms and woodpeckers and flowers whose names I forget; and I did not care if I never smelt petrol again.

SCIENTIFIC INTERESTS OF CHILDREN

SIR,

Mr. Rallison's article in your February issue on the science interests of children contains much information of great interest and importance to science teachers, but there are two points on which I feel that some critical comment is necessary.

Firstly, in Fig. 2, interests in biological topics are all put in one group, yet physics is subdivided into electricity, mechanics, light, heat, sound and magnetism as though these were each equivalent to the whole complex subject Biology—or the whole of Chemistry. If the interest in these six branches of Physics are added together, we shall get a fairer picture and that picture will show that a school science course in Physics, Chemistry and Biology, with a little Astronomy and Geology, will cater adequately for the interests of children of the age range covered by Mr. Rallison's evidence. This is just the kind of course recommended by the Science Masters' Association and called 'General Science' to emphasise its breadth in contrast to the old, narrow type of course which only amounted to, say, Physics and Chemistry for boys, and Botany for girls.

This brings me to my second criticism. Mr. Rallison repeats the frequent mistake of thinking that the term 'General Science' necessarily precludes the separate study of, say, Physics, Chemistry and Biology. It is true that, in the hands of the right teacher, science can to some extent be treated as one subject, but subdivision is likely to be necessary in most cases. Realising this, the Committee of the Science Masters' Association has emphasised that "a school is, in fact, teaching General Science even if its pupils study Physics, Chemistry and Biology separately under different teachers and some Geology and Astronomy as part of their Geography. It is the increased breadth of the Science course that we advocate under the name of General Science and we are content to leave the details of the teaching of such a course to the individual schools." (*School Science Review*, June 1943).

Yours, etc.,

HUGH P. RAMAGE,
Hon. General Secretary, Science Masters' Association.

Mr. Rallison has sent the following reply to Mr. Ramage's comments on his article:

Mr. Ramage grumbles at my placing all biological topics in one group yet subdividing physics. This was because I am primarily a Physics-Chemistry teacher and, when carrying out the investigation on which my article is based, I was also collecting data as to *relevance* of the 'balance' of child interests in physics-branches (like electricity, mechanics, etc.) to the still quite common practice in Grammar Schools of preparing pupils for school certificate in parts only of physics like Magnetism and Electricity, or Heat, Light and Sound. It was only by analysing the content of physics into branches that I could prove that the children's interests spread over 'General Physics' and to my mind this condemns sectional physics treatment.

I was not so particularly concerned with biological teaching problems and for the purposes of my investigation I was satisfied merely to point out the undeniable fact that a respectable body of biological interests exist and draw the relevant inferences to the place that Biology should be given in our Science Teaching. As a member of the Science Masters' Association, I am quite familiar with their official ideas on General Science and I can't see how the kind of course indicated by my recommendations is essentially contrary to S.M.A. 'doctrine'.

Mr. Ramage's second criticism puts me in the situation of trying to defend something I neither wrote nor implied! He says, I "repeat the frequent mistake of thinking that the term 'General Science' necessarily precludes the separate study of, say, Physics, Chemistry and Biology." I fail to see where my thinking in any part of the article shows this. The nearest approach to this implied idea from Mr. Ramage was in the section, ". . . there is much dislike of General Science among science teachers. In some quarters there is already a tendency to revert to the more specialised separate sciences. . . . It is felt by many science masters that those going on in the Sixth Form . . . should start laying their foundations of study and experience in the special subject. On the other hand, science masters with this outlook often regard General Science as suitable fare for the type for whom Bernal has expressed concern."

The context of this quotation makes it obvious that I am quoting a cross-section of opinions from science teachers, not my *own* opinion on these matters.

My own position is that of an investigator of opinions and my article goes on to say "In an attempt to reconcile some of these difficulties, a number of investigations have been carried out . . ."

THE BOOKSHELF

Penicillin—A Dramatic Story. By Boris Sokoloff (London, Allen and Unwin; pp. 167; 7*s.* 6*d.*).

THIS is an English reprint of an American book, and therefore inevitably suffers from being already out of date and from being based, at least so far as the pioneer work in this country is concerned, on hearsay evidence and the very limited amount of information which has been permitted to appear in the technical journals.

As the author states in his preface, there is an undisputed need for a popular account of the discovery, development and, above all, the use of penicillin. The value of this particular book in meeting the need is, however, doubtful, to a large extent because the author has too literally made *dramatic* the operative word in his title. At the same time his critical judgment of published papers is often poor; a case in point is his ready acceptance of Karl Meyer's theory on the relationship between biotin and lysozyme which, advanced only tentatively by Meyer himself, has since proved impossible to substantiate. There are also several errors in fact, in chronological order, and in interpretation.

Nevertheless, despite his weakness for superlatives such as 'amazing', 'startling', and 'stunned', the author has produced a text which has at least that quality of readability which is so essential for a popular book; his is a journalistic rather than a technical success. In the absence of any other popular account in book form a good deal may be forgiven; the book will probably find a ready sale.

Perhaps the most valuable feature of the book is its bibliography of about 350 references to original publications, though there is nothing later than 1944.

TREVOR I. WILLIAMS

A Scientist in the Criminal Courts. By Dr. C. Ainsworth Mitchell (Chapman and Hall, London, 1945; 144 pp., 19 plates; 8*s.* 6*d.*).

To analytical chemists, students of scientific criminology and DISCOVERY readers, the name of Ainsworth Mitchell needs no introduction and his book no further recommendation. One imagines that he wrote this new book with the lay public especially in mind, for it is of general interest although it documents fairly fully a number of court cases in which the scientific evidence was of paramount importance. Forgeries, income tax frauds, 'questioned documents', poison-pen letters, secret writing and invisible inks, each of these subjects has one or more chapters to itself.

What Industry Owes to Chemical Science. (Heffer, Cambridge, 3rd edition, 1945; pp. 372, 18*s.*)

THIS book has grown out of a series of articles published in *The Engineer* during the Great War and reprinted in book form in 1918. Since the last edition (1923) it has been much enlarged, general supervision over its preparation being exercised by the publications committee of the Royal Institute of Chemistry. About fifty aspects of the industrial application of chemistry are covered by as many contributors, all of whom are experts on the subject they write about. The book, which is easy reading, gives a useful introduction to industrial chemistry and there are useful bibliographies at the end of each section. With a 22-page index it has a value as a reference book if one needs to find brief details about the landmarks in any particular branch of industrial chemistry: a few sections give tables of industrial output, the one on dyestuffs being particularly good in this respect.

Far and Near

Selective Weedkillers

The first issue of FARMING, sister journal to DISCOVERY that deals with the technical and scientific aspects of agriculture, contains a very interesting article by Professor G. E. Blackman of Oxford University on selective weed control. It traces the history of chemical weed control from 1896, when M. Bonnet, a French expert on vineyards, noted that when vines were sprayed with Bordeaux mixture the spray drift injured some of the annual weeds, notably yellow charlock. Copper sulphate was therefore tried out as a spray, and was the first chemical compound found to be capable of killing one plant without injuring another—it killed off charlock growing in a cereal crop. The number of selective herbicides grew slowly: after copper sulphate came ferrous sulphate, sodium nitrate and ammonium sulphate, as well as powdered kainit and cyanamide. In 1911 Rabaté, another Frenchman, published an account of the use of dilute sulphuric acid for weed control in cereal crops. In Britain, Professor Blackman and Dr. W. G. Templeman showed that the suppression of aggressive annual weeds like white charlock by means of this acid could sometimes treble the yield of cereals. Meantime in Germany copper nitrate was found to be more effective than copper sulphate. (A 1% copper nitrate spray was the equal of 5% copper sulphate.)

1932 saw another important advance—again made in France. G. Truffaut and I. Pastac discovered that certain dyestuffs already used as insecticides—dinitrophenols and dinitrocresols—killed annual weeds. In France and United States sodium DNOC (dinitro ortho cresol) came on to the market as a herbicide. The American L. E. Harris found DNOC became more effective with the addition of small quantities of ammonium sulphate.

War gave an impetus to this line of research in Britain for there was much tumble-down grassland to be ploughed up and many dormant weed seeds that would spring into life and depress corn yields; moreover weedkillers would be invaluable at a time when farm labourers were scarce. Spraying sulphuric acid proved its worth against onions, the young seedlings of which stood up to the acid even better than cereals. Only on light gravel soils did the treatment reduce the onion crop.

From work on plant hormones grew the studies that showed 2-methyl-4-chloro-phenoxy-acetic acid (MCPA or methoxone) and 2 : 4 dichloro-phenoxy-acetic acid (DCPA) to be effective weedkillers. The early researches in this connexion were carried out by two teams, one at Jealotts Hill and the other at Rothamsted, and these chemicals were included in the general programme of weed control trials made by a third team, directed by Professor Blackman.

Other articles in the first number of FARMING are: Future of British Grassland (Sir George Stapledon), Pollination and Fertilisation (M. B. Crane), National Institute of Agricultural Engineering (S. J. Wright), Rural Reconstruction (C. S. Orwin). The journal appears every second month, and costs 1*s*. 6*d*. a copy (1*s*. 7*d*. inclusive of postage). The yearly subscription is 9*s*. 6*d*. All inquiries should be sent to The Empire Press, Norwich.

Scientific Manpower Report Due

DISQUIET is being expressed in many quarters about the breaking up of several Government research teams that played a vital part in the winning of the war. One establishment in process of shrinking is the Royal Aircraft Establishment; it was reported in March that 40% of the staff have received redundancy notices. The Scientific Manpower Committee is due to publish its first report any time now, and it is to be hoped that the Government will see fit to publish it so that everyone can get a detailed and balanced picture of the way scientific manpower is being allocated in the conversion from war to peacetime economy. At present it is not possible to tell whether allegations about muddle are justified or not.

The Value of Food Yeast

THE possibility of supplementing the British diet with food yeast, a rich source of vitamins and protein, was considered in Britain during the war when the blockade threatened our food supplies. A pilot plant to make food yeast was erected in the Government's Chemical Research Laboratory at Teddington, but large-scale production never became necessary owing to the success with which the Royal Navy countered the U-boat attacks and no industrial food yeast plant ever came into operation. In Germany, on the other hand, food yeast was manufactured, as happened in the 1914-19 War; according to one report, a single German plant—at Wolfen—turned out 1000 tons of food yeast a year after 1939.

Results of feeding experiments with food yeast have just been published in the Medical Research Council's War Memorandum No. 16, entitled *Food Yeast: A Survey of its Nutritive Value* (Stationery Office, 3*d*.) One trial that is recorded was carried out in a village school in Oxfordshire. To the diet of 79 children was added a biscuit containing 10 grams (just over one third of an ounce) of food yeast, while the control group received a similar biscuit but without any food yeast in it. During a test period of ten weeks the children of the 'yeast' group put on an average of 1·7 lb. as against 1·2 lb. in the control group. In a five-months' trial (with 40 children in both groups) average increases were respectively 4·4 and 3·1 lb. This work confirmed the results of other trials in which a small daily dose of food yeast was found to improve the nutrition of school children.

Half an ounce of the dried yeast is ordinarily regarded as the maximum adult daily dose; a quarter of an ounce may be taken without risk of 'digestive disturbances', states the report.

Food yeast promises to be a valuable supplement to the diet of colonial peoples lacking in vitamins and first-class protein. It is one of the richest known sources of B vitamins (including vitamin B_1, riboflavin and nicotinic acid) and contains 50% of protein. (Incorporated in a diet of which the protein is otherwise derived mainly from cereals food, yeast protein has a nutritive value approaching that of milk proteins.) There is interest therefore in the result of a trial carried out in Nigeria where it was found that natives with symptoms of B_2 vitamin deficiency improved strikingly or were completely cured after 5-7 weeks' treatment with $\frac{1}{4}$-$\frac{1}{2}$ ounce of food yeast daily.

Incidentally this report, from the nutritional standpoint, quotes 6*d*. a pound as a likely cost for food yeast. In chemical engineering circles the figure generally discussed is nearer 1*s*. a pound, but this point will not be settled until the large food yeast plant in Jamaica is fully producing.

200 German Scientists for Industry

THE Board of Trade stated last month that 200 German scientists and technicians were being brought to Britain. The Lord Chancellor, speaking in a short debate on 'Provisions for Security', talked about "300 or 400 highly skilled Germans" coming over. Presumably the discrepancy in numbers is explained by the German scientists introduced by Service departments, whereas the Board of Trade is interested in the industrial application of scientific and technical knowledge.

French Counterpart of "Discovery"

THE first issue of a new popular science monthly has been published in Paris. The title of the journal is *Atomes*, and its editor is Dr. Pierre Süe of the Collége de France. The first two numbers of *Atomes* are carrying the radar article by Sir Robert Watson-Watt, reprinted from the September 1945 issue of DISCOVERY

Biology in the Universities

THE University Staffs Committee of the Association of Scientific Workers has prepared *A Memorandum on the Teaching of Biology in Universities*. This has been published as a 24-page pamphlet, price 1*s*.

University Grants to Increase

THE Chancellor of the Exchequer is asking Parliament to vote £9,450,000 for university grants for 1946-7, £3,800,000 more than the sum voted in 1945-6.

N.P.L. Tests of Volumetric Glassware

THE Metrology Division of the National Physical Laboratory has prepared a new edition of the pamphlet, *Tests on Volumetric Glassware*. Any reader who would like a copy should apply to The Director, N.P.L., Teddington, Middlesex.

Death of Dr. F. W. Lanchester

THE death occurred on March 8 of Dr. F. W. Lanchester at the age of 77. Dr. Lanchester, who will always be remembered for his pioneer work on aerodynamics, built the first British petrol automobile. He began construction of an experimental motor car in 1894, and seven years later the first 10/12 h.p. Lanchester car was marketed. It was during those seven years that he began the studies which were to revolutionise the theory of flight. In 1895 he read a paper to the Birmingham Natural History and Philosophical Society in which he put forward the conception known as the Vortex (or Circulation) Theory of Sustentation in Flight, but it was not until he published the first volume of his book *Aerial Flight* (1907) that the great importance of his theories was recognised. In the meantime, the circulation theory had become known as the Prandtl theory, after Dr. Prandtl of Gottingen, who had worked along similar lines to Lanchester, of whose work he was aware and to whom he gave full credit.

When the Advisory Committee on Aeronautics (forerunner of the Aeronautical Research Committee) was set up in 1909 he became a member, and he served on this committee until 1920.

He was an F.R.S. Among other honours he held the James Watt Medal of the Institution of Mechanical Engineers, the Gold Medal of the Royal Aeronautical Society and the Ewing Medal of the Institution of Civil Engineers.

Crowther Leaves British Council

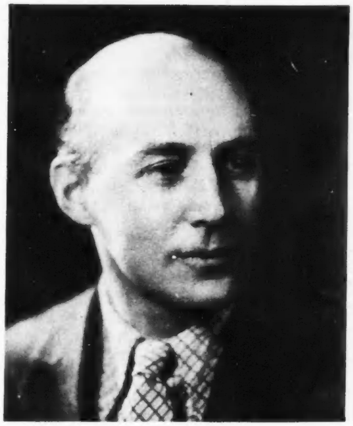
MR. J. G. CROWTHER has resigned his post as director of the Science Department of the British Council, which he has held since 1941. Prior to that date the Council had virtually ignored the achievements of British science; presumably it was the war which led to the appreciation in Hanover Street of the importance of science and the necessity of reflecting the progress of British science if the Council's task of presenting a true picture of 'the British way of life' was to be fulfilled. In less than five years Mr. Crowther succeeded in building up a Science Department, which is today one of the most flourishing and vigorous divisions of the British Council. 1941 was not an auspicious year for starting such a venture, and it says much for Mr. Crowther's drive and organising capacity that it grew so rapidly, branching out into four main sections: *General Science*, which was Mr. Crowther's special concern (apart from his directorial work), *Engineering* (with Professor S. L. Davies as consultant), *Medicine* (under Dr. N. Howard Jones, who edits the excellent *British Medical Bulletin*) and *Agriculture*, the youngest of the sections and with Dr. W. T. H. Williamson in charge.

Dr. F. W. Lanchester (1868-1946)

Mr. Crowther was directly responsible for starting the news sheet, *Monthly Science News*, though the expansion of his department soon made it necessary for him to delegate the editing of *M.S.N.* He also played a big part in establishing the Society for Visiting Scientists, of which he became secretary.

The breadth of the Science Department's activities was well displayed in the recent 24-page brochure that described in outline the work the department does. The growth of the department to its present state when it has a staff of about twenty is the more remarkable since it occurred mainly in war years.

Dr. Howard Jones is now acting director of the Department.



J. G. Crowther

Scientific Aspects of Kew

THE importance of the herbarium at Kew has been increased by the complete destruction of the Berlin Herbarium and the partial destruction of that at Vienna. This point was made by Sir E. J. Salisbury, Director of the Royal Botanic Gardens, Kew, in a lecture to the Royal Society of Arts on February 19. The total number of types in the Kew collection probably exceeds 200,000, he said. The species grown at Kew number 40,000, invaluable material capable of providing data on habit, biology, physiology and structure. Extensive researches are carried out in Kew's Jodrell Laboratory on the systematic anatomy of flowering plants and many of the results would be published in a voluminous work incorporating the knowledge already collected by Solereder.

Kew has sent out many consignments of seeds and young plants, not only to British possessions where botanic gardens were already established, such as those of Jamaica, Trinidad, Cape Town and Durban, but also to foreign countries. Kew has supplied seeds of tung oil plants since 1928, when it was decided to attempt to establish the plant in the British Empire; the results can be seen in the flourishing plantations now yielding oil in Nyasaland, Swaziland, the Union of S. Africa and Upper Burma. The speaker also spoke of the export of dried potato sets that weigh less than a fifth of the normal. (An account of the method, which permits of carriage by air, was published in DISCOVERY in June 1944, p. 162-3). Many trials have shown that the technique yields a crop little smaller than that obtained in the ordinary way.

An antibiotic with agricultural potentialities was mentioned at the end of the lecture. Dr. Portheim working in the Jodrell Laboratory has found that substances with marked antibiotic properties can be obtained from actinomycetes of the genus *Trichoderma*. (Actinomycetes are organisms that have some characterbtics of fungi, and in other ways resemble isacteria; they are placed in a separate order on their own. In the last issue there was a note about another antibiotic of actinomycete origin—streptomycin). When organisms of this genus are grown in an appropriate medium, a liquid is produced that is highly inimical to the particular fungus responsible for the deadly Panama disease of bananas. Dr. Thaysen, director of the Microbiological Station in Trinidad, is to make field trials in the hope that the material may prove capable of combating this hitherto uncontrollable disease.

Symposium on Textiles and Dyeing

THE Society of Dyers and Colourists will hold on May 23-25, in Leeds University, a symposium on dyeing, colouring and textiles. Many eminent scientists in Great Britain, Australia, U.S.A., and the Continent have promised original contributions.

The Society has been able to secure a certain amount of hotel accommodation in Leeds, Bradford and Harrogate, but early application will be necessary to Mr. H. Foster, the Honorary Secretary of the Symposium Committee, at the Society of Dyers and Colourists, 32-34, Piccadilly, Bradford, England.

From War Office to Coal Board

SIR CHARLES ELLIS, professor of physics at King's College, London, and scientific adviser to the Army Council since 1943, will be a member of the National Coal Board that the Government is setting up. It is anticipated that Sir Charles will be in charge of the research side of the Board's activities. An authority on radioactivity, he was one of Rutherford's team at the Cavendish, and was joint author with Rutherford and Chadwick of the book *Radiations from Radioactive Substances*.

Scientific Film Review

THE defeat of Nazi Germany has been attributed largely to the success of Allied strategy in the Middle East. The Allied political and economic plan and its likely effect on the future of this great area and its huge population is brilliantly described in the film entitled *Today and Tomorrow*. Together with the vast amount of information and objective reporting which is presented, there is in the film deep understanding of these peoples, of their outlook and attitude to life.

Although man first learned to till the soil and raise a crop in Palestine, thousands of years of unscientific farming reduced much of the Middle East to barren desert. Soil erosion by wind and flowing water has removed the precious top soil, leaving bare rock and stones which yield poor crops and little grass for goats, sheep and cows. Planned irrigation and the successful use of fertilisers had just started to be effective in the Nile basin in the pre-war years, but cotton was the essential crop and the whole Middle East area was dependent for food upon imported products. In 1941, with the German armies advancing on Stalingrad and menacing India and Rommel hard upon the Nile delta, imports of food into the Middle East countries were cut by 86%. Adding to these food shortages were the millions of fighting men drafted to defend the area.

As a short term policy all existing foodstuffs were collected and distributed in zones according to the local need. This in itself was an unusually difficult problem because of the inherited mistrust of the Arabic peoples, who thought that once their granaries were under (to them) alien control they would never see any recompense. This situation was aggravated by subtle propaganda from enemy radio stations.

To implement a long-term policy, the organisation called the Middle East Supply Centre was set up. With the vast majority of the people starving and undernourished, M.E.S.C. attacked the problem scientifically. To reclaim millions of acres of barren land a programme was started of educating the farmers in modern methods of soil-conserving farming; and trained officials directed the planting of grasses and trees to hold the top soil. Many of the small streams and rivers were controlled to retain their alluvial products, and the water was used to irrigate the land in the drier seasons. The rivers, dams and vast recultivated valleys are shown in some exceptionally fine pictorial photography, which is a pleasing change from the heavy soot-and-whitewash effects so common in films shot in brilliantly sunlit countries.

The Egyptian water-buffalo plays an all-important part in the feeding of the community. The plentiful milk of this animal, and cheese, form a large part of the daily diet, while its dung is used as a fertiliser; artificial manures were not available through shortage of shipping space. The Egyptian Government instituted an insurance scheme for these animals, for the fortune of a family was entirely dependent on its existence and milk production. With this insurance scheme, a plan was put into operation for improving the strain of the cattle. Government breeding centres issued new cows as required by the inhabitants participating in the insurance scheme, and all bulls of inferior strain were either killed or castrated. This sequence is particularly good and a great amount of information and statistics is effectively presented by sound and pictures.

The sequences outlined above show the structure and content of this film, but far more important is its message—that more than thirteen nations successfully collaborated in reorganising the Midde East during the war. The film shows clearly how the United Nations food plan, worked out at the Hot Springs Conference, was actually put into practice with success, making one ask why nations at peace cannot collaborate as they did in war.

The film in its closing sequences shows the methods employed for the extermination of the locust swarms so destructive to the crops. The campaign disregarded man-made frontiers; it covered thirteen countries and exterminated the locusts in their breeding places. One of the most significant shots in the film shows a British-made aeroplane spraying a locust-killing dust produced in America with equipment made in Russia.

The film is available in 16 mm. and 35 mm. sound, and is distributed by the Central Film Library.—DEREK STEWART.

(*This review is contributed by arrangement with the Scientific Film Association.*)

Night Sky in May

The Moon.—New moon occurs on May 1d 13h 16m, U.T., and full moon on May 16d 02h 52m. The following conjunctions take place:

May			
3d 02h	Venus in conjunction with the moon	Venus	2° N.
6d 05h	Saturn ,,	Saturn	2 S.
7d 10h	Mars ,,	Mars	2 S.
13h 01h	Jupiter ,,	Jupiter	3 S.

The Planets.—Mercury, which is in superior conjunction with the sun on May 31, is too close to the sun during the month to be favourably observed. Venus is conspicuous in the western sky, setting 2 hours after the sun on May 1 and 2h 20m after the sun on May 31. At the beginning of the month the portion of the illuminated disk visible from the earth is 0·93 and at the end of the month it is 0·86. Viewed through binoculars or a small telescope the planet looks nearly like the moon when she is almost full but appears very much whiter. Mars can be seen in the earlier portion of the night, setting at 1h 48m on May 1 and at midnight on May 31. Saturn is easily recognised near Mars and sets about an hour earlier than Mars. Those who are interested in comparing the magnitude of one star with that of another should try to estimate the magnitudes of the planets during May. That of Mars averages 1·3 g of Jupiter −2, and of Saturn 0·4. Jupiter sets later than the other two planets, the times being 4h 07m and 2h 09m at the beginning and end of the month respectively. As Jupiter and Saturn will soon be unfavourably placed for observation, any who are interested in the features of the planets should use the opportunity to study the motions of Jupiter's four largest satellites and the ring system of Saturn.

There is a partial eclipse of the sun on May 30, invisible at Greenwich.

Amongst a number of interesting spring constellations may be noticed Lyra. The bright star Vega, which is about 26 light-years distant from the earth, is one of the most brilliant stars in the sky, and is about 50 times as luminous as the sun. Near it is the star ε which a very keen eye can recognise as a double, but a small telescope shows that each component is itself a double so that ε Lyrae is a quadruple star. The Ring Nebula in Lyra, between β and γ, can be seen through a small telescope. It looks like a ring of smoke and consists of gas in a very rarefied condition. β Lyrae is a variable star, its magnitude fluctuating between 3 and 4 in a little under 13 days. It is not necessary to use a telescope to observe the changes in this star's magnitude as they can be easily followed by the naked eye. The variation is due to the revolution of one star around another, or, to be more correct, to the revolution of both stars around their common centre of gravity, the smaller of the two bodies being brighter than the larger. Corona Borealis is a small but beautiful constellation whose resemblance to a crown is obvious at a glance. It can be found by producing the line joining γ and η Ursae Majoris. It is famous for its new star which blazed out in 1866 but which quickly faded and became a mere ninth magnitude star. On February 9 it was observed by different people to be a star of magnitude 3, having suddenly brightened up again after a lapse of 80 years.

The first in Great Britain to discover it was N. Woodman, a boy of fifteen, of Newport, Mon., who reported his discovery to the Astronomer Royal.

This time it faded much more quickly than it did previously and at present it is somewhere near a ninth magnitude star again. It may break out again in the near future, but this is rather improbable. The outer layers were blown off the star with terrific force and at Yerkes Observatory the spectroscope showed that the debris was moving out from the star with a speed of 2,000 miles a second, though the speed estimated by British astronomers a few days later was considerably less than this. Some internal convulsion, the nature of which is not fully known, is responsible for these temporary stars or novae.

M. DAVIDSON, D.Sc., F.R.A.S.

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